An experimental investigation of obsessive compulsive disorder.

Jakes, Ian C

An Experimental Investigation of Obsessive Compulsive Disorder

Ian C. Jakes

This thesis is submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy at the Institute of Psychiatry, University of London.

June 1992

To my parents, Charles and Ruth Jakes

The best lack all conviction, while the worst
Are full of passionate intensity

W B Yeats
"The second coming"

Abstract

The thesis is divided into four parts. In Part A, what are termed the "standard diagnostic criteria" for Obsessive-Compulsive Disorder" (OCD) are reviewed, and argued to be implausible. The beginnings of an alternative approach to the definition of the disorder are presented. Empirical investigations of the characteristics of obsessive-compulsive experience among OCD sufferers are also reported. The results of these investigations both confirm the critique of the "standard diagnostic criteria", and are consistent with previous phenomenological investigations of OCD patients. Judgment is suspended as to the full importance of these diagnostic and phenomenological considerations to the understanding of OCD.

In Part B, a number of theoretical approaches to OCD are discussed, including behavioural/learning accounts, "Pavlovian" personality theories, Janet's account, a "cybernetic" approach, an account from a psychodynamic perspective, and the "cognitive-structural" theory. It is argued that none of these approaches is able fully to explain the phenomena associated with OCD.

In Part C, the "cognitive-structural" theory of OCD is tested empirically. Three investigations are reported, none of which provide strong support for this theory.

In Part D, an attempt is made to pick out, from the approaches considered earlier, any ideas which may offer some hope of progress in the understanding and/or treatment of OCD. The suggestion which is examined to this end, made by several of the accounts considered above, is that the unassertive behaviour of some OCD patients may be an important precipitant of their symptoms. Evidence relevant to this claim, and its implications for treatment, are reviewed. It is suggested that this approach may offer some insights and useful suggestions for some cases of OCD. Some suggestions are offered as to further work which might be conducted along these lines.

Acknowledgments

My thanks are especially due to Mr Gerry Riley, Dr David Hemsley (who supervised the thesis), Dr Tim Dalgleish and Dr Phillipa Garety. Thanks also to Professor Isaac Marks and Dr Robin Jacoby for allowing me access to their patients, Professor Jeffrey Gray, Mr Charlie Sharp, Mr Eric Glover, Dr Graham Dunn, Professor Brian Everitt, Mrs Morgan Randall-Vining and her colleagues at 115 Denmark Hill, all of the staff on Tyson West II, Mr Andrew Clay and his flatmates, Ms Johanna Beyts (and Ben), Mr Richard Whittington, Mr Simon Jakes, and Mr Padmal de Silva (who also supervised the thesis).

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Part A: Natural History, Definition and Phenomenological Studies of Obsessive Compulsive Disorder

Chapter one: The natural history and definition of OCD

1.1 Natural history

There are a number of summaries of the evidence available regarding the natural history of Obsessive-Compulsive Disorder (OCD) (for example, Black 1974, Rachman and Hodgson 1980, Reed 1985), and the findings reported in these summaries will not be repeated in detail here. OCD has traditionally been regarded as relatively rare, its prevalence among psychiatric out-patients being estimated to be between 0.3% and 0.6% (de Silva 1988); its prevalence among in-patients is somewhat higher, but still under 5% (Black 1974, Hare et al 1972, Rachman and Hodgson 1980). The prevalence of the disorder among the general population had been estimated to be about 0.5% (de Silva 1988), although the recent American Epidemiological Catchment Area (AECA) Studies found a "strikingly high lifetime prevalence...ranging from 2% to 3%" (Bebbington 1990), with a 6 month prevalence of between 1.3-2.0% (de Silva 1988). Bebbington (1990) has studied the prevalence of OCD in a community in London, making a more detailed appraisal of the severity of the illness than that made by the AECA surveys. His lifetime prevalence estimate was broadly in line with that of the AECA surveys, although Bebbington emphasised that most of the cases he identified were relatively mild, and he suggests that this may in part be why the prevalence of the disorder is so low in the psychiatric population.

The AECA studies also found that males were affected by OCD in almost the same numbers as females, that the disorder was most commonly reported in the 25-40 year age group (with a median onset of 24 years), that OCD was more common among whites than blacks and that the disorder was equally common in all social classes. Onset of OCD after the age of 45 is rare (Black 1974, Rachman and Hodgson 1980).

Some further details concerning the natural history of OCD are offered elsewhere, in particular what is known about the factors which precipitate it (see section 3.1), the contribution of genetic and "social learning" influences (see section 3.1), and the relationship between OCD and the obsessional personality and personality disorder (see sections 3.4 and 4.12).

1.2 OCD: existing suggestions for the diagnostic criteria

1.2.1 Introduction

What follows is a critique of a number of definitions of OCD which are currently on offer. The distinction between OCD and "normal" obsessions and compulsions (Rachman and

de Silva 1978) will not be examined, this distinction being determined only by the relatively vague judgement as to whether or not the disruption in a person's functioning caused by his obsessions and/or compulsions is sufficiently severe for the symptom to be judged to be of clinical severity. While the diagnostic criteria which are to be examined in this section and section 1.3 will be referred to as those for OCD, they are, therefore, equally relevant to the task of identifying, among sub-clinical experiences and behaviour, those which are "normal" obsessions and compulsions. There will also be in this section no discussion of the definition the "obsessive-compulsive personality" (Pollack 1979, 1987, Reed 1985) although some comments on this topic are offered later (see section 4.12).

1.2.2 The major authorities

Snaith (1981) remarks that "since the time that Esquirol first described a case and Morel first used the term obsession (Black 1974), obsessional neurosis has been subject to frequent redefinition". Other writers disagree; Rachman and Hodgson (1980) suggest that since the introduction of the concept of "obsessional neurosis" its definition has "produced little controversy". Reed (1985) also takes this line, and argues that all the major authorities in this area - Westphal, Janet, Freud, Jaspers, Schneider, Lewis etc. - agree on the definition of the disorder (see Reed 1985, p1-4, for quotes from these authorities). Reed suggests that the same definition is also put forward by DSM-III (although see section 1.2.4).

Thus, according to Reed, all of these sources suggest that for any thought, act, impulse, image etc. to be symptomatic of OCD, it must (1) have a "subjectively compulsive quality" - the person must feel "compelled pressed or driven" to think or act as he does, (2) be recognised by the person as "senseless or absurd" - insight is retained into the morbidity of the thought or action and (3) be resisted by the person - he must fight not to think or act as he does (quoted passages from Reed 1985, p4, emphases in original).

1.2.3 Lewis on the definition of OCD

While Snaith appears rather to exaggerate the extent of disagreement regarding the diagnostic criteria for OCD, Reed overstates the extent of agreement (although see his 1985 discussion p5-6). Lewis (1936), for example, states that Schneider's definition is at once "precise and practicable" (p325) but goes on to deny, against Schneider, that senselessness is essential, and further suggests that resistance is both essential and omitted by Schneider's criteria.

Attention must be drawn to the sense in which Lewis uses the term "resistance" in the frequently quoted passage from his 1936 paper. He says: "...there should...be mention of the feeling that one must resist the obsession. This resistance is experienced as that of one's free

will. The innumerable devices, rituals, and repetitions of the obsessional are secondary expressions of this immediate experience; they carry into effect the urge to ward off the painful and overwhelming obsession. The more overwhelming and painful the obsession, the more urgent and unsuccessful the devices to ward it off" (Lewis 1936, p325). Thus, the "devices, rituals and repetitions" of these patients are part of their resistance, according to Lewis; he is not arguing that these behaviours have themselves to be resisted.

This is in sharp contrast to the criteria presented by Reed, which state that for such behaviours to be symptomatic of OCD they must be resisted. This use of "resistance" by Lewis is evidently confusing. Consider, for example, the ritual checking of a plug socket in response to the obsessional doubt that the plug has not been removed. Such checking does not resist, or "carry into effect the urge to ward off", this doubt. On the contrary, it is a failure to resist it or to ward it off.

Reed's criteria are not alone in misrepresenting Lewis on this point. For example, Stern and Cobb (1978) in their comments on Lewis's views take issue with his diagnostic criteria because of the response of their sample of OCD patients to "...probing questions [which] were asked to determine whether the patient struggled against an internal resistance, or conversely just gave in and carried out the ritual activity" (1978, p235). On Lewis's use of "resistance", these questions would be incoherent.

1.2.4 The DSM III/III-R, and Rachman and Hodgson's, criteria: the separate definitions of "obsession" and "compulsion"

The definitions of "obsession" and "compulsion" are separated by DSM III and DSM III-R and Rachman and Hodgson (1980). All of these sources restrict the former term to those OCD symptoms which might be termed "involuntary" or "passive" (de Silva 1988, p196) phenomena and the latter term to those which might be termed "voluntary" or "active" (de Silva 1988, p196) phenomena. These same sources also suggest a number of other additions to the criteria presented by Reed.

To illustrate the distinction between "obsession" and "compulsion" which is being drawn, a checker's thought that his door may be unlocked would, on this distinction, be regarded as an obsession, his act of checking the door as a result of this thought would be regarded as a compulsion. Although neither DSM III nor DSM III-R is explicit on the point, their definitions do not require compulsions to be overt behaviour. Thus, mental activity which is "purposeful and intentional", for example, counting to oneself to avoid misfortune, is compulsive according to their definitions. Compulsions may be, then, either overt (that is, behavioural) or covert (that is, mental), while obsessions are always covert (that is, mental),

according to these definitions.

DSM III defines "obsessions" as "...recurrent, persistent ideas, thoughts, images, or impulses that are ego-dystonic, that is, they are not experienced as voluntarily produced, but rather as thoughts that invade consciousness and are experienced as senseless or repugnant. Attempts are made to ignore or suppress them". This passage is slightly modified in DSM III-R, which states that obsessions need only be initially experienced as "intrusive and senseless" (p245). The DSM III/III-R criteria add that a person must recognise his thoughts and impulses etc. to be the product of his own mind if these are to be symptomatic of OCD. Rachman and Hodgson (1980, p2) similarly suggest that the diagnostic criteria for obsessions are "intrusiveness, internal attribution, unwantedness and difficulty of control". A variation on this also used by these authors is that obsessions are "intrusive, unwanted thoughts, images or impulses which are unacceptable and/or unwanted". Rachman and Hodgson add that "internal resistance, and rejection of the idea...as alien and/or unrealistic" are "confirmatory indicators" of the idea in question being an obsession.

Turning now to the separate definition of "compulsions" offered by these sources, DSM III-R defines these as "repetitive, purposeful, and intentional behaviours that are performed in response to an obsession, according to certain rules, or in a stereotyped fashion" (p245). It adds that such behaviour must be "performed with a sense of subjective compulsion that is coupled with a desire to resist the compulsion (at least initially)" and that "the person [must recognise] that his or her behaviour is excessive or unreasonable" (p245). DSM III-R also requires that patients "do not derive pleasure from carrying out [a compulsion], although it provides a release of tension" (p245). (Lewis [1936, p326] similarly notes that the more the doing of an act is enjoyed, the less that act is like a compulsion.) Rachman and Hodgson (1980, p2) offer very similar criteria for the definition of "compulsion".

The three criteria presented by Reed, along with the distinction between obsessions and compulsions and the other additions suggested by both the DSM criteria and Rachman and Hodgson's definition, will be referred to as the "standard" diagnostic criteria in what follows. (See Walker [1973] and Snaith [1981] for interesting alternative approaches to the definition of OCD.)

1.2.5 The ordinary uses of "obsession" and "compulsion"

It is worth noting in passing that the distinction made between the uses of the terms "obsession" and "compulsion" by DSM-III/III-R criteria and Rachman and Hodgson (1980) is stipulative, the employment of "obsession" and "compulsion" in ordinary (non-psychiatric) discourse presenting no such distinction. Purposeful activity by which a person is preoccupied

to an unusual degree, for example stamp collecting or jogging, may be described as that person's "obsession", while strong urges, for example, to drink or smoke, are described as "compulsions", despite their not being in themselves purposeful or intentional behaviour.

Other distinctions are also apparent among the ordinary uses of "obsession", "compulsion" and related locutions such as "being obsessed by", or "feeling compelled to do", something. One may be described as "feeling compelled" to do anything one is strongly inclined to do, for example, acting on a moral principle, smoking a cigarette, reading something of great interest. The activity in question might be unusual, irrational or unwanted, but equally it might not. To say that somebody "feels compelled" to do something is neutral as to these points. This is not true as regards somebody's "having an obsession with", or "being obsessed by", some thought or activity. One would not say this of a person's, for example, commitment to a moral principle, if one shared this commitment with this person. The same appears to be true of somebody's "having a compulsion" to do, or "being compulsive about" doing, something. These also imply something unwanted or irrational about the desire or behaviour etc. in question.

1.2.6 Evaluation of the standard defining criteria

1.2.6.1 The DSM criteria: stipulation vs. description

To begin with a few remarks specifically to do with the DSM-III/III-R criteria, it might be suggested that the definition offered by these sources stipulates rather than describes the diagnostic criteria for OCD, in contrast to those suggested by the other sources cited above. This is the point of manuals such as DSM-III and DSM III-R, it might be argued - they are intended to instruct clinicians, not to give an account of what they do. On this view, the question of the accuracy of the DSM criteria would clearly not arise (where the "accuracy" of a given set of criteria is understood to be how good a description of an existing concept these criteria provide). In what follows it will be assumed that this suggestion is incorrect. This is to assume that if the decisions of experienced diagnosticians do not meet the DSM criteria, this is bad news for these criteria, not the diagnosticians. This assumption, then, gives the DSM criteria the descriptive status shared by all the other sources mentioned above, and means that the question of the accuracy of these criteria can be raised.

The exception to this is the distinction the DSM criteria draw between "obsession" and "compulsion" which is, as has already been pointed out, stipulative. This distinction is useful in highlighting an important distinction among the symptoms of OCD and is on these grounds justified. This distinction will be adopted in the following discussion, and all criticisms and discussion of the accuracy of the DSM criteria should be understood as excluding it.

In what follows, it will be argued that the decisions of experienced diagnosticians do indeed not square with the standard diagnostic criteria which have been outlined above. It is important to be clear that this is not to claim that most diagnosticians, if asked, would agree that these criteria do not give a good account of their decisions. Quite probably they would say that these criteria do agree with their diagnostic practise, or would at least regard them as giving an account of this practise which is sufficiently accurate to be useful for practical purposes. But it will be argued in what follows that if one examines which patients are and are not actually classified as suffering from OCD, it can be seen this classification is <u>far</u> more subtle and complex than the standard criteria allow, to the extent that these criteria cannot be regarded as providing even a rough approximation of the concept. It is, of course, neither a paradox nor a surprise that at least some diagnosticians will not themselves be aware that this is so. The ability to use a concept correctly does not imply the possession of an explicit account of that use.

The standard defining criteria will be evaluated first as regards obsessions and then as regards compulsions. This will, of course, mean two separate evaluations of the three criteria presented by Reed which do not distinguish between obsessions and compulsions.

1.2.6.2 Obsessions

If the very similar definitions of "obsession" suggested by DSM III-R and Rachman and Hodgson (1980) are put together with the three criteria from Reed's discussion, the list of diagnostic features provided is as follows: (a) a "subjectively compulsive" quality, (b) intrusiveness, (c) unwantedness/unacceptability, (d) difficult to control, (e) repetitiveness, (f) persistence, (g) internal attribution/recognised as the product of own mind, (h) recognised as senseless/rejected as unrealistic or alien and (i) resisted/attempts made to ignore/suppress. These nine criteria, it is to be argued, fail to provide the sufficient conditions for a definition of "obsession". Some of them, it will be further argued, are not exhibited by every instance of OCD and are in this sense also not necessary for the diagnosis.

Consider first the claim that these features do not provide the <u>sufficient</u> conditions for this definition. These features do not distinguish obsessions from, for example, many phobic experiences. Take the example of an agoraphobic approaching a crowded shop. He may well experience such thoughts as "I may lose control and panic", "I may be sick", "I may go mad", "I may faint" etc. He may also experience images which concern these same themes. It is not being claimed - and nor is it necessary for the present argument to claim - that these phobic thoughts and images will <u>always</u> meet <u>all</u> of the criteria (a)-(i) above . All that needs to be shown and what is indeed being suggested, is that such experiences will <u>sometimes</u> do so

while still being recognisably part of a person's phobic difficulties. Thus, the patient's phobic thoughts that he may, for example, panic and lose control will - working down the list (a) to (i) - frequently be such that in this situation he feels compelled to think them while finding them intrusive, unwanted/unacceptable, difficult to control, repetitive and persistent. He will always recognise them as the product of his own mind, and will at least sometimes also recognise them as senseless and resist them.

Going beyond this particular example, it is difficult to think of anyone experiencing any significant degree of distress - be it anxiety, worry, anger, depression or whatever - without that person's thoughts at least frequently possessing features (a)-(f) from the list above. The person being non-psychotic arguably guarantees that his thinking will possess feature (g) (also see section 1.3.11) while the features (h) and (i) are merely different ways in which a person may struggle with what he is thinking (Lewis 1936). Yet, whatever else we may mean by calling some aspect of a person's experience an "obsession", it is clear that we must mean by this something more specific than that he has some non-psychotic distress with which he is struggling. It may be concluded, therefore, that the criteria considered above must be rejected as not providing the sufficient conditions for a definition of "obsession".

What, then, of the claim that some of these features are not exhibited by every instance of OCD and are thus not necessary? A number of authors, for example, Stern and Cobb (1978) and Walker (1973), report a proportion of patients whose symptoms lack features (h) (senselessness) and (i) (resistance) and yet who have been classified by senior diagnosticians as suffering from OCD (also see chapter 2 below). Against such findings it might be pointed out that DSM III-R qualifies the necessity for senselessness and resistance, claiming that these need only be present initially. Yet this claim seems to be made in the absence of any evidence that subjects who do not report senselessness and resistance would in all cases have once done so and would not have otherwise received the diagnosis of OCD. The same objection may be used against Reed's (1985, p5-7) related reply that insight and resistance are both a matter of degree.

Consider now feature (e) (repetitiveness). To be told that, for example, a mother's impulse to harm her child lacks this feature does not enable one to conclude that this impulse is not an obsession. It seems that such impulses might indeed only occur once on any particular occasion, and that one would not, by virtue of this alone, wish to withhold a diagnosis of OCD.

1.2.6.3 Compulsions

Turning now to compulsions, that is those OCD symptoms which are voluntary or

"active" behaviour, the three standard criteria from Reed's discussion once again fail to provide the necessary and sufficient conditions for a definition. Thus, these criteria cannot be sufficient because, for example, many patients will report, as part of a phobic difficulty, that they feel compelled to escape from their feared objects while at least sometimes trying to resist this feeling and also at least sometimes regarding it as senseless. Resistance and senselessness are also not necessary features of "compulsion" - the work of Stern and Cobb (1978) and Walker (1973) quoted above is again relevant here (also see chapter 2). Once again, DSM III-R qualifies the necessity for these features and suggests that compulsions need only be resisted initially, but, as with the similar claim made regarding "obsession", this suggestion is unsubstantiated.

What of some of the additional features for the definition of "compulsion" which are suggested by DSM III-R to be necessary - that compulsions must be performed in response to an obsession, according to certain rules or in a stereotyped fashion? Do these take us any closer to a satisfactory definition of "compulsion"? It would seem not. While compulsions certainly may be performed according to certain rules or in a stereotyped fashion, both of these features would appear to be absent in many patients. Indeed, the DSM III-R definition may itself be consistent with this observation in that it seems to be a disjunction rather than a combination of these three features which is being put forward as necessary for a diagnosis of OCD.

To be successful, therefore, the definition requires its third feature to be exhibited by all of those compulsions which fail to exhibit the other two features. But this third feature - that compulsions are carried out in response to obsessions - is uninformative, given that the urge to perform a compulsion is itself an obsession. One is not told, that is, how to distinguish urges to perform compulsive actions from urges to perform other kinds of behaviour. This distinction presupposes a definition of "obsession" which has not as yet been provided either by DSM III-R or the other sources quoted earlier.

1.2.7 Implications of these criticisms

Raising these doubts as to the defining criteria for OCD is no mere semantic exercise. Substantial questions may be begged in both research and therapy by what the phenomena which are being investigated and treated are understood to be. In order to illustrate this claim, consider a contribution to the literature on OCD by Rachman and Parkinson (1981). It seems that this contribution makes some important errors, and that it does so because of the definition of OCD with which its authors are working. This definition is that suggested by Rachman and Hodgson (1980), which was quoted among the standard diagnostic criteria

discussed above. Error results in this case from treating these criteria as providing the sufficient conditions for a diagnosis of the disorder; quite different problems may arise from regarding these criteria as necessary (see section 4.7).

Rachman and Parkinson (1981) present the following anxious thoughts (among others), all of which were reported by mothers concerning their children who were at the time in hospital for surgery: "Ever since I knew she was booked in I have been thinking what might go wrong with the anaesthetic and surgery..." (the same mother is reported as having a "repetitive image" in which "I have seen her lying there like a vegetable and not coming round from the operation"), "I have this repeated image of K. on a trolley, and they put him in a bed and then I see the blood everywhere. I try hard to clear it from my mind. The image frightens the life out of me.", "Its been on my mind the whole time. I haven't been able to stop myself thinking about him and his operation...", "I was thinking about it all the time. I was constantly going through in my mind how I would explain things to him. I keep seeing him in his gown, asleep, being taken down the theatre. I've seen him go down in my mind", "I keep hearing him and seeing him in hospital, crying" (all quotes from Rachman and Parkinson 1981, p115 and p117).

It should be noted that these thoughts would be likely not meet one of the criteria specified by the standard definitions of OCD, that of senselessness (de Silva 1989). The women in Rachman and Parkinson's study, that is, would presumably regard their thoughts of their sick children as entirely reasonable material to have on their minds. Otherwise, however, these thoughts and images would at least very often satisfy the remaining criteria suggested by the standard definitions of OCD. Thus, referring back to the list of defining criteria for "obsession" presented earlier, these thoughts would usually have a "subjectively compulsive" quality and be experienced as intrusive, unwanted or unacceptable, repetitive and persistent. These thoughts would similarly be difficult to control, be recognised by these women as the product of their own minds and attempts would, finally, also be made by these women to resist (that is, to ignore or suppress) these thoughts, as indeed was explicitly stated in some of the quotes above.

It is <u>because</u> the thoughts reported by these women do largely satisfy the standard definitions of OCD that these authors are lead to regard them as a <u>normal analogue</u> of OCD - to regard them, in other words, as "normal obsessions" (Rachman and de Silva 1978). Thus, Rachman and Parkinson say as regards these thoughts that "the present study is designed to extend our comprehension of obsessions along what might be described as a normal dimension" (Rachman and Parkinson 1981, p111).

Having taken up this position, Rachman and Parkinson then attempt to use the close study of the thoughts and images experienced by these women to shed light on clinical obsessions. For example, they argue that the absence of depressed mood as a precipitant of these thoughts is evidence that this mood state may have had its importance as a precipitant of clinical obsessions overstated.

Against this position, however, is it not plausible to argue that these thoughts and images are not obsessions at all? These women are clearly anxious about their children, but nothing which has been said so far suggests that there is anything obsessional about this anxiety at all. In particular, and with the earlier criticisms of the standard definitions in mind, this is not suggested by the fact that these thoughts are experienced as repetitive, intrusive, unwanted etc. No inference from the nature of these thoughts to that of obsessions (clinical or otherwise) is, therefore, possible - or at least no such inference is possible without begging the question of whether or not non-obsessional anxiety and obsessions differ such that the inference from the one to the other is misleading.

This is perhaps not as powerful an example as one would like of the standard defining criteria leading to error - as was pointed out above, the thoughts and images in this example do not meet all of these criteria. They do, however, meet all but one of them, and these criteria would thus lead one to suppose that these thoughts and images are at least very similar to symptoms which may be diagnosed as OCD. What the earlier critique of the standard criteria makes plain here, then, is that these thoughts and images are no more similar to obsessions than they are to many non-obsessional phenomena. Indeed, further argument would surely show that there is a far greater similarity between these thoughts and images and some phobic states than there is between them and obsessions.

1.3 A different approach to the definition of OCD

1.3.1 Introduction: the use of intuitive tests

In trying to argue in the previous section that the characteristics of senselessness and resistance are not exhibited by all OCD symptoms, it was possible to refer to empirical work which shows that some patients who have been classified by senior diagnosticians as having OCD do not report these characteristics as regards their symptoms. No such empirical evidence is available to support the analysis of the concept of OCD which is to follow. It will thus be suggested that a number of characteristics are necessarily exhibited by various instances of OCD, without these suggestions being backed up by evidence that they accord with actual diagnostic practise. The argument will depend instead upon an appeal to what Hare (1981) has termed "linguistic intuition". The term "intuition", acknowledged by Hare himself to be a

"dreadful and dangerous word", must be used with caution. What is meant by it in this context is only that it is possible to recognise, while having as yet no explicit account of the diagnostic criteria for OCD, what are and what are not instances of the disorder. In attempting to produce an account of the diagnostic criteria for OCD (or rather, as regards the present discussion, the beginnings of an account), it is therefore possible to proceed by exploring what one is able to recognise "intuitively" as being instances of the disorder. One may then attempt to infer from this recognition the basis on which - the implicit criteria in accordance with which - these instances have been so recognised.

A source of potential difficulty for this approach is that one is required in adopting it to assume that there will be a general agreement in the judgments of different diagnosticians as to what are and what are not instances of OCD. If a sufficient degree of intuitive disagreement were found to exist among diagnosticians, then the whole approach of appealing to intuitive tests would collapse, and it would have to be concluded that there is no established concept of "OCD" at all. Until any evidence of such radical intuitive disagreement is found, however, one is entitled to proceed with the use of "intuitive tests" on the tentative assumption that such evidence will not be forthcoming. It is worth noting in passing that this approach of using "intuitive tests" has also been used above without having been explicitly introduced there as such. For example, an appeal to "intuition" was made in arguing that much thinking and behaviour which can be recognised as phobic meet (and thus call into question) the standard defining criteria for OCD.

1.3.2 No unitary criteria for OCD

What, then, are the defining criteria for OCD? It is suggested that the best response to this question is a refusal to answer it in that form. Philosophical work, most notably that of Wittgenstein (1953), rejects the assumption that there <u>have</u> to be common and peculiar characteristics shared by <u>all</u> the phenomena picked out by any given term. This assumption is made as regards the concept of OCD by the standard definitions considered above, and in the present discussion it is intended to show that this assumption is indeed mistaken in this case. Thus, the central claim will be that <u>features which are not common to all OCD symptoms will often play a part in determining that some symptoms are of that character.</u>

1.3.3 The limits of the present account of the definition of OCD

It is not suggested that the examples to be given below provide anything like an exhaustive account of <u>all</u> of the features which have a part to play in defining thinking and behaviour as OCD. The following discussion of the concept of OCD will instead focus mainly on the distinction between OCD and phobias. Although this is but a part of the definition of

OCD, it seems that even this part of the definition is as yet not fully understood and the following examination of this distinction is again not intended to be exhaustive. It will be followed by a few remarks as to the distinction between OCD and delusions.

While having thus acknowledged that the following discussion will not deal with the distinction between OCD and many non-phobic disorders, it is worth remarking that an examination of the distinction between OCD and some anxiety symptoms which would probably be in at least some cases regarded as non-phobic will be included. Examples of non-OCD symptoms of this kind which are to be discussed in what follows are such fears as whether or not a close relative has come to harm and whether or not one is about to have a heart attack (it is of course not being claimed that such symptoms are not diagnosable as OCD by virtue of their involving such themes). As Cobb (1986) notes, "diagnostic systems discriminate clearly between disorders involving phobic avoidance such as agoraphobia [on the one hand] and generalised anxiety where such focal problems cannot be identified [on the other]". Where such fears of heart attacks and relatives having come to harm etc. are not associated with the avoidance of particular situations, therefore, they would tend not to be regarded as phobic symptoms, on Cobb's account. To this extent, therefore, what follows will address the distinction between OCD and some non-phobic anxiety states. Indeed, as Cobb also notes, some authors have suggested that much or all apparently "free floating" anxiety is in fact precipitated by thoughts such as these; if this view is correct, the inclusion of these thoughts will mean that the following discussion will include a great deal of non-phobic anxiety. There is, however, no necessity here to assume that this is the case - it is readily conceded that there may be much non-phobic anxiety the distinction between OCD and which lies beyond the following discussion. Fears such as whether or not one is about to have a heart attack or whether or not a relative has come to harm will for the sake of simplicity be referred to as "phobic" in what follows.

1.3.4 The distinction between phobias and OCD

1.3.4.1 Introduction

A number of features which play a part in distinguishing OCD from phobic states are to be presented. To reiterate the central claim made above: for each of these features, it will be argued that (1) its presence is crucial to the distinction between phobic states and those instances of OCD which exhibit the feature in question, and (2) there are other instances of OCD symptoms which do not exhibit this feature at all.

Two points are worth stressing at the outset of this discussion. Firstly, it will for the purposes of the present argument only need to be suggested for each of the features to be

presented below that its presence is part of the reason for characterising those symptoms which exhibit it as OCD. Thus, a series of examples of OCD symptoms will be presented for each of which it will be pointed out that it exhibits a given feature in contrast, it will be claimed, to phobic symptoms. But this is not to claim that the OCD symptom may be characterised as such by virtue of its exhibiting the feature in question alone. The symptom is, as noted above, being presented as recognisably OCD on intuitive grounds, and attention is only being drawn to the feature which distinguishes that OCD symptom from phobic states. It may well be that other, non-phobic emotion or behaviour, can also be distinguished from phobias in the same way without being symptomatic of OCD. How such non-phobic phenomena would be distinguished from OCD would clearly have to be determined on other grounds. But it is not necessary for the purposes of a discussion which is concerned only with the distinction between phobias and OCD to explore what these other grounds will be (there will nonetheless be reason for making just a few remarks concerning this question towards the end of the discussion).

Secondly, it is not being suggested that no patient diagnosed as phobic will ever report any thinking or behaviour which exhibits the characteristics to be discussed below. The claim is rather that, insofar as such thinking and behaviour are reported, the patient is having experiences or is behaving in ways which are not phobic in nature. A phobic diagnosis will be inadequate if and only if such experiences or behaviour are a sufficiently important source of distress for the patient.

1.3.4.2 Characteristic (i): symptoms which concern the exercise of one's own will

OCD symptoms may involve the sufferer in fearing that he may knife somebody, utter some obscenity or do some other unacceptable thing. Sometimes an OCD patient will alternatively report fears as to terrible things which he thinks he may have already done.

Phobic symptoms may also involve, as these OCD symptoms do, a fear of unacceptable things one may do - an agoraphobic may report as part of his phobic state a fear that he will be sick, faint, panic etc. But this is not to fear one's own future or past intentional action as in the above examples of OCD symptoms. It seems that such a fear of one's own intentional behaviour is by definition not phobic, and this feature is consequently sufficient to distinguish those OCD symptoms which exhibit it from phobic states.

Rather than fearing what they might do or might have done, some OCD patients experience impulses to perform unacceptable actions. It seems that one can once again feel sure that such experiences are not phobic in character - there are, that is to say, by definition no phobic impulses of this kind.

1.3.4.3 Characteristic (ii): "covert object" of fear or discomfort

As noted above (see section 1.2.6.2), phobics will frequently experience intrusive, repetitive and unwanted thoughts and images of a distressing nature, for example, an agoraphobic approaching a crowded shop may experience thoughts of how he might be sick or faint in the shop, images of these things happening etc. How might such a patient's thoughts and images of what he thinks is going to happen to him be distinguished from such obsessions as the following: (a) images of horribly mutilated dead bodies, (b) blasphemous thoughts, (c) unacceptable insults concerning, for example, the appearance or conduct of the patient's partner or close relatives, and (d) number sequences and nonsense phrases which continually run through the patient's mind.

What seems to be the crucial difference between such experiences as these and phobic thinking is that the latter refers to objective states of affairs which the patient fears he will have to encounter and/or things which he fears may happen - often his fear will concern situations which he knows he will have to encounter very soon as in the example of the agoraphobic considered above. To take some further examples, the phobic patient may similarly think of or picture the spider which he knows he is going to have to confront, or imagine the heart attack which he thinks he is about to sustain or the humiliation he thinks he is about to suffer in some social situation.

OCD thinking of the kind being considered in the present section is not like this - the distress it involves is not the result of its referring to possible or actual situations which the patient thinks might or will occur; the patient is distressed by the content of his thinking without its referring to possible or actual situations at all. In this sense, then, the patient's fear or discomfort may be said to have a "covert object" as suggested in the title above.

Thus, in the case of examples (b), (c) and (d), and in contrast to the examples of phobic thinking considered above, it is not the truth or possible truth of what has been thought which distresses the patient. Indeed, it is not even logically possible for number sequences and nonsense phrases to be true or false and the same applies to many blasphemies and insults too - for example, a patient to be discussed below had insulting, blasphemous thoughts which consisted only of swear words addressed to Jesus Christ.

The distress in examples (b) and (c) seems rather to be a matter of these thoughts being regarded by the patient as the kind of thing one really ought not to think. If such thoughts occur frequently, furthermore, the sheer frustration and worry of having such unwanted matter constantly on his mind also seem likely to be contributing to the patient's distress, and in the case of example (d) this seems likely to be the heart of the problem.

Example (a) is somewhat different, but even here the patient is not experiencing what phobic patients sometimes do, such as an image of a close relative dead at home while having intense anxiety that the relative has indeed died. The images experienced in the present example are unaccompanied by such anxieties as this, and distress the patient solely by virtue of their horrifying contents - they are not taken by the patient to represent any situation he thinks will or may come about. As with examples (b), (c) and (d), therefore, the distress involved does not have to do with the question of whether or not something will happen or will have to be confronted etc.

A phenomena which may seem to be closely related to these examples is exhibited by those OCD patients who respond to their feared situations by performing some <u>covert</u> activity such as counting to themselves rather than by carrying out - in contrast to many phobics - some overt behaviour such as escaping from the feared situation. But some phobics may also try to deal with their fears by taking "covert action", for example, telling themselves that there is nothing of which they should be scared. What seems to secure an OCD, as opposed to a phobic, diagnosis for some covert behaviour is thus not its being covert as such but rather its being prompted by superstitious thinking as to what effect the covert action in question will have, for example, thinking that a feared situation may be rendered safe by the performance of the covert action. Rather than being included in the present section, therefore, such instances of OCD have to be included in the section which follows.

1.3.4.4 Characteristic (iii): superstitious and bizarre thinking

Some OCD patients report anxieties about outcomes which strike one not merely as rather unlikely but <u>bizarrely</u> unlikely, for example, removing small stones from pathways for fear that others will trip on them and come to serious harm, carefully positioning books on shelves in case they should fall and badly hurt someone etc.

It is not, of course, being denied that phobics do sometimes fear unlikely outcomes, for example, heart attacks and strokes may be feared by phobic subjects who have every reason for believing themselves to be perfectly healthy in physical terms. What is being claimed is that by definition one does not encounter among phobic states the bizarre degree of unlikelihood exhibited by these OCD symptoms. This distinction seems as if it might not be a categorical matter, which would render the difference between phobic states and those OCD symptoms which are to be distinguished from them in this way a matter of degree rather than kind.

Some justification should briefly be provided here for the claim that fears of having heart attacks or strokes do not involve the same degree of irrationality as do fears of people

coming to great harm as a result of tripping on tiny stones or having books fall on them from ordinary household shelves. This is shown to be so by its being common to know of others who have suffered the kinds of misfortune which preoccupy the phobic patients in these examples; the same clearly does not hold true for the examples of OCD preoccupations considered under the present heading. Nonetheless, there may undeniably be difficulties in the reliable assessment of how bizarre any given thought is (Kendler et al 1983) and it must be conceded that, to the extent that this is so, any distinction which depends upon the assessment of this may be difficult to judge satisfactorily.

It may be easier to judge other cases in this category which involve not merely a greater degree of irrationality on the part of the OCD patient but rather his behaving in an entirely superstitious manner, for example, positioning objects in a certain way in order to avoid bad luck or - to reintroduce the covert behaviour example referred to above - counting to oneself to neutralise the potential threat perceived in some situation. Phobic avoidance and escape appear by definition not to involve superstitious thinking - the patient may certainly entertain irrational ideas as to the outcome of his behaviour, such as thinking that he will avoid a heart attack or being sick in public by, for example, escaping from a crowded shop. But while irrational, such behaviour seems plainly to fall short of being superstitious.

1.3.4.5 Characteristic (iv): repetitive behaviour

Some OCD patients carry out repetitive checks of, for example, plug sockets and light switches, doing so because they are unable to convince themselves either that they have carried out their earlier checks properly or that they have carried these checks out at all. Similarly, objects may be cleaned over and over again. While phobics certainly do sometimes experience repetitive thoughts, as has been argued above (see section 1.2.6.2), repetitive action of the kind being considered here seems by definition not to be phobic in nature.

1.3.4.6 Characteristic (v): arrangement of objects

Some OCD patients report themselves to be unable to feel comfortable unless things in their environment are arranged in a set way - for example, unless all objects of a given kind are lined up or all books and pieces of paper placed so that they are parallel to the edges of the tables on which they have been placed. Sometimes superstitious thinking may accompany such practises, in which case the behaviour would be classified under category (iii) above. But not all patients do seem to carry out behaviour of this kind for such reasons - some report simply not being able to stand things being arranged in any other fashion.

Such discomfort is, once again by definition, not phobic. Phobic patients fear types of situations, for example, crowded shops, social interaction etc. But these situations are never

identified by the exact arrangement of objects in them. Similarly, phobics may often fear objects or animals, but it is once again always the mere presence of the objects or animals not their arrangement or exact position which troubles these patients.

1.3.5 The independence of characteristics (i)-(v)

The central claim of the present argument does not only require that these various features do distinguish OCD symptoms from phobic states. It also requires that these features are independent of one another, and are thus entirely different ways in which OCD symptoms are distinguished from phobic states. It seems clear this is the case. Some of these features may of course sometimes occur together and be parts of the same symptom - for example, a patient may perform a superstitious ritual as a result of experiencing an impulse. But there is absolutely no necessity for this. It follows that the feature on the basis of which one case of OCD may be distinguished from phobic difficulties need not be exhibited at all by another case of OCD (and thus obviously play no part in distinguishing that case from phobic difficulties). It may be concluded that the central claim of the present discussion (see section 1.3.1) has been established - features which are not common to all OCD symptoms play a part in at least many cases in determining that some symptom is of that character.

Indeed, it seems to be logically impossible for some of the features which have been considered above to form parts of the same symptom. The concerns of a patient whose symptoms exhibit characteristic (ii), for example, by definition do not involve his worrying whether or not some event has taken place or will take place. This means that this symptom cannot also be a doubt that he has performed some terrible act or a fear that he may or is about to do so. His symptom can thus not exhibit characteristic (i).

1.3.6 Connecting themes between characteristics (i)-(v)

There is, then, no single feature or collection of features shared by all OCD phenomena by which they may all be distinguished from phobic states. This is not to deny, however, that there are themes which are shared by at least some of the various features which have been discussed. Those symptoms included under categories (iii)-(v), for example, may all be said to be examples of various kinds of ritual. The concept of "ritual" evidently behaves much like that of "OCD", with those features which are crucial to the characterisation of some behaviour as ritualised not being exhibited at all by other examples of ritual behaviour.

Another characteristic is shared by many of the different kinds OCD symptom which have been presented above, and this may be best introduced by first noting that all phobias may be satisfactorily described as fears of, or discomfort about, some external object or situation (the term "external" is being used here as it was in the discussion of category [ii]

above - the bodily states which trouble some phobic patients are thus "external" in that they are not mental events). In the case of many OCD symptoms this description is, by contrast, unsatisfactory - it omits, in various different ways, some crucial aspect of such symptoms. This, then, is another link between such OCD symptoms - they cannot be satisfactorily described as fears of, or discomfort about, some external object or situation. The most obvious example of OCD symptoms which cannot be so described is those which exhibit characteristic (ii), which by definition do not involve external objects or situations at all. Symptoms exhibiting characteristic (i) are somewhat less obvious examples in that they clearly do, by contrast, involve such situations (those which involve being with the person concerning whom the patient experiences his impulses and doubts about having done some harm etc.) But it is surely unsatisfactory simply to describe the patient as having a fear of the presence of the person concerned, because this suggests that there is something about that person the patient fears. The problem is not a fear of a certain situation - the person in question being present - but rather a fear the patient has concerning himself, and in particular of how he will behave (or may have behaved) in that situation.

Some of the symptoms which exhibit characteristic (iii) involve superstitious thinking, for example, positioning objects in a certain way in order to avoid bad luck. One might certainly say of a patient who exhibited such a symptom that he has a fear of objects not being positioned in a certain way, but this description once again omits the very heart of the symptom - the patient's magical thinking concerning the consequences of his arranging these objects in the manner he does.

Similarly, it is unsatisfactory to describe at least much of the repetitive checking which exhibits characteristic (iv) as a fear of, or discomfort about, leaving (or the possible consequences of leaving) light switches on, plugs in sockets, doors unlocked etc. This description once again omits the central feature of the symptom - the repeated attempts of the patient to convince himself that light switches are off, doors locked etc. Repetitive checking of this kind implies some further difficulty other than fear or discomfort concerning the task in question (see section 3.2.5).

By contrast, however, three remaining examples of OCD symptoms may indeed be satisfactorily described as fears of or discomfort about external objects or situations. Thus, consider those symptoms exhibiting characteristic (iii) which involve concerns about bizarrely unlikely outcomes - to repeat the earlier examples, concerns such as whether or not people will come to serious harm through tripping on small stones or having books fall on them from household shelves. These kinds of symptom may be described perfectly accurately as a fear

of or discomfort about these things happening. This description does not leave out any crucial aspect of the symptom, in contrast to the examples considered above. The same applies to symptoms exhibiting characteristic (v) which may be satisfactorily described as fears of or discomfort about, for example, all objects of a given kind being lined up or all books and pieces of paper being placed so that they are parallel to the edges of tables. It may also be possible to argue the same point as regards at least some of the repetitive cleaning which exhibits characteristic (iv) (see section 3.2.5), in contrast to the repetitive checking of plug sockets or light switches discussed above.

All three of these kinds of OCD symptom, therefore, may be satisfactorily described as fears of or discomfort about some external object or situation, in contrast to the symptoms discussed earlier. Staying with this notion of an "external object or situation", however, it is possible to formulate in these terms a theme which is shared by all of the various kinds of OCD symptom identified in this discussion. All of those symptoms which cannot, in contrast to phobic states, be described as fears of or discomfort about external objects or situations may also be said, more vaguely, to involve a <u>different role</u> for external objects and situations than do phobic states - this more vague statement clearly follows from the other. But this more vague statement is at least usually applicable to the three kinds of OCD symptom which <u>can</u> be described as fears of or discomfort about external objects or situations. Patients with symptoms which exhibit characteristic (v), and patients who exhibit the repetitive cleaning which exhibits characteristic (iv), do not remove themselves and do not have to have anything removed from their environment in order to reduce their discomfort - the repetitive cleaners act to remove germs which are not actually present at all, while patients suffering from symptoms exhibiting characteristic (v) merely rearrange their surroundings without removing anything from them. The actions of both of these kinds of patients are therefore in clear contrast to the behaviour undertaken by the phobic patient, which will involve either his removing himself from some fear provoking situation or having his phobic object removed from it.

Turning finally to symptoms which exhibit characteristic (iii), the role played by external objects and situations in at least some of these is perhaps closest to that played by them in phobic states - the patient may remove, for example, the small stones from pathways which are the source of his fear or discomfort. But even here the patient's concerns are what might be termed "more remote" from the situations which provoke them than are the phobic's. These OCD symptoms involve the exercise of a great deal more <u>imagination</u> concerning possible dangers in the patient's situation than do the far less unlikely outcomes which disturb

phobic patients. To this extent, therefore, one may once again assert that the role of external objects or situations in this kind of symptom is different from that found in phobic states.

One can adopt an alternative approach to this issue of there being a different role for external objects and situations in the case of category (iii) symptoms. This approach is suggested by the fact that the patient is able in the examples considered to remove or reposition for himself the stones and books which are the source of his fear or discomfort. His problem does not involve him in being unable to touch or be near any object or having to escape from or avoid any situation. Indeed, far from wanting to escape, the patient will feel obliged to remain in the situation which makes him uncomfortable so that he can neutralise what he perceives to be the danger within it. This, then, appears to be an entirely separate way in which this kind of OCD symptom features a different role for external objects and situations than do phobic states. The latter will involve the patient in being unable to be near certain things or in having to escape from or avoid certain situations.

Indeed, the same point can be made as regards a number of the other types of OCD symptom. Category (v) symptoms, and at least much of the repetitive checking in category (iv), do not usually involve the patient in having to escape or avoid anything, nor his being unable to be near to or touch some object. These symptoms too, therefore, may also be said to feature a different role for external objects or situations in this way as well as in those identified earlier.

It needs finally to be noted that this point cannot be made as regards still other types of OCD symptom. Those symptoms in category (i) which feature unacceptable impulses, for example, will usually involve the patient in being unable to bear being in certain situations, that is, those which provoke his impulses and in his consequently having to escape from or avoid those situations.

To sum up this discussion of the distinction between phobic states and OCD: in a variety of different ways OCD symptoms may be distinguished from phobic states in featuring what may be termed a different role for external objects and situations. The foregoing discussion in particular says nothing as to how far OCD might be distinguished from non-phobic disorders, for example, depressive illnesses, other anxiety disorders (although note the comments in section 1.3.2) and disorders involving delusions, concerning which some remarks are to be made below.

1.3.7 The criterion of senselessness

It has been explicitly stated in the foregoing discussion that each of the features considered need not be sufficient for those symptoms which exhibit them to be classifiable as

OCD.These features are sufficient to distinguish OCD symptoms from phobic states, but this leaves open the question of whether or not any of these features are shared by some non-phobic phenomena which are not instances of OCD. The question of what else might be required to justify a diagnosis of OCD was earlier set to one side as unnecessary for an account of the distinction between OCD and phobic states.

The question is, however, nonetheless one to which it is worth paying a little attention as it has some bearing on the critique of the traditional diagnostic criteria for OCD offered earlier (see section 1.2.6). Consider again those OCD fears which concern the exercise of one's own will. This experience of fearing how one may act is clearly not by itself sufficient to justify a diagnosis of OCD. Plainly, there are instances of people feeling scared about what they might intentionally do, or of experiencing impulses to do actions which they find unacceptable, without these experiences being either OCD symptoms or their sub-clinical equivalents. As a result of extreme anger with another person, for example, someone might say that he is scared of the harm he thinks he might do to that person. This experience is readily distinguishable from that reported by the OCD patient - especially if the angry person's fear concerns only later regrets he may have or legal punishments he may face as a result of his doing harm to another, while his immediate impulse is quite unequivocally to act in precisely this manner. It seems that the feature of senselessness is required with symptoms of this kind before they begin to take on an OCD aspect. The very desire to do harm must itself strike the patient as inappropriate or alien (note that resistance is not so important to this distinction - the angry person in the example above may resist his aggressive impulses by virtue merely of his fear of later regrets or legal sanction).

It was argued above (see section 1.2.6) that there are some OCD symptoms - the rituals of many of these patients, for example - which are not experienced as senseless and that this feature is thus not necessary for a diagnosis of OCD. Placing OCD symptoms such as these rituals side by side with those involving aggressive impulses, it is clear that the role of senselessness in the diagnosis of OCD is similar to that of the characteristics (i)-(v) discussed above. While it plays, that is, no part in defining some symptoms as OCD it is crucial in defining others as such. Senselessness may also be contrasted with the five characteristics discussed earlier, however, in that it does not form any part of the OCD/phobic states distinction. Aggressive impulses, that is, are not phobic symptoms whether or not they are experienced as senseless.

1.3.8 The distinction between obsessional and psychotic thinking

Reed (1985, p9) argues that the absence of reported insight in psychotic delusions

means that obsessions may be "readily differentiated" from them. He suggests that deluded subjects, by definition, "lack insight [and find] nothing strange or senseless in their often palpably absurd ideas". As Reed puts it, the distinction between delusions and obsessions is a matter of form rather than content - it is a matter of how the patient thinks about something (the degree of insight he exhibits etc.) rather than what he thinks about, according to Reed.

Snaith also points out that "the theme of the retention of insight into the morbid nature of the idea" has been traditionally seen as central to the distinction between a neurotic and a psychotic symptom (1981, p85). He goes on to quote Kraupl-Taylor who makes a most interesting contribution in tracing this distinction back to demonological theories and "the distinction between possession and obsession; in the latter, the evil spirits surround the individual (Latin: obsidere, to besiege) whereas in possession the spirit enters into its victim and dominates him completely" (Snaith 1981, p85, original emphases). Thus "possession" is here presented as the counterpart of the modern notion of "delusion", where this notion is understood necessarily to involve - in contrast, it is claimed, to the state of being obsessed - the patient having no insight into the morbidity of his thinking. It consists, that is, of his having been completely "taken over" by his morbid thinking.

Yet the critique of the standard diagnostic criteria for OCD (section 1.2.6) suggests that, contrary to Reed's position and the traditional account of neurotic pathology outlined by Snaith, the absence of reported insight cannot distinguish all obsessions from delusions. While it has been noted above that senselessness is indeed crucial in defining some symptoms as OCD, this feature is not shared by all instances of the disorder.

Further difficulties for Reed's position may be raised by those patients who are classified by some diagnosticians as "partially deluded". These patients are held to have beliefs which would otherwise satisfy the criteria for delusions but do not hold these beliefs with absolute conviction: "partial delusions are expressed with doubt, as a possibility which the subject entertains but is not certain about" (Wing et al, quoted in Garety 1985, p29).

1.3.9 A content-based distinction between obsessions and partial delusions/delusions?

How, then, are obsessions to be distinguished from partial delusions and how are those cases of OCD where reported insight is absent to be distinguished from delusions? Perhaps this is, in Reed's terms, sometimes a matter of the content rather than the form of the patient's thinking, contrary to Reed's suggestion?

Consider Mullen's (1979) approach to the definition of delusions, which has been described as "representative of present-day British and American views" (Garety 1985, p29). Mullen appears, early on in his discussion, to agree with Reed when he says: "a delusion

is...judged to be present more from the manner in which it is adhered to and the reason for its emergence [that is, its "form" in Reed's terms] than any aspect of content" (p29). Mullen also notes, however, that this distinction between form and content is easier to maintain regarding disorders of perception than disorders of belief, and he goes on to include, among other characteristics "usually attributed" to delusions, that "their content is often fantastic or at least inherently unlikely" (p36, my emphasis). (Mullen also notes that a true belief may be a delusion, and this might at first be thought to conflict with his criterion of the belief being fantastic or inherently unlikely - but such a case might presumably still meet this criterion by the true belief being held for bizarre reasons.)

Regarding the question of the distinction between delusions and obsessions, Mullen himself, like Reed, regards the 'formal' characteristics of the patient's thinking as crucial. But as such characteristics have already been questioned, how successfully might this distinction be instead drawn, contrary to Mullen's own account, in terms of his characterisation of the contents of delusions ?

Two points suggest that Mullen's content based criterion would not be sufficiently specific to distinguish delusions from obsessions. Firstly, as has been argued above, at least some obsessions seem to have "bizarre" contents and indeed very many seem to involve a concern for "at best inherently unlikely" matters. Secondly, DSM III-R distinguishes between bizarre and non-bizarre delusions, the latter "involving situations that occur in real life, such as being followed, poisoned, infected, loved at a distance, being deceived by one's spouse or lover..." (DSM III-R, p202). This evidently amounts to a denial that all delusions have to be fantastic or inherently unlikely, and it may therefore be concluded that Mullen's content-based criterion could not be used to distinguish obsessions from delusions - some obsessions appear to meet this criterion while some delusions do not.

1.3.10 Ego boundaries

Can more specific contents than those mentioned by Mullen, therefore, be used to distinguish some delusions from obsessions? What if one considers only the type of thinking which DSM III-R defines as "bizarre delusions", for example, the belief that thoughts are being removed from one's mind ("thought withdrawal"), the belief that thoughts which are not one's own are being inserted into one's mind ("thought insertion") and the belief that one's thoughts, as they occur, are being broadcast from one's mind to the external world so that others can hear them ("thought broadcasting")? All of these examples share the theme of the patient's "ego boundaries" being blurred - his own mental states are misidentified as public phenomena and/or as belonging to somebody else. Are these ego boundary confusions

sufficient for the person who suffers them to be both (1) not diagnosable as OCD, and (2) diagnosable as deluded - or at least as partially deluded should this confusion be expressed as a possibility rather than a certainty?

It is question (1) - the question of whether or not bizarre thinking of the "blurred ego boundary" variety is sufficient to disqualify a person's thinking as being symptomatic of OCD - which is of most relevance the present discussion. A patient included in the studies below (see chapter 2 and Part C) who was diagnosed as OCD shows that it is possible for a patient exhibiting certain kinds of ego boundary confusion to receive this diagnosis (the patient does not exhibit either thought insertion or thought broadcasting so her case does not count against the suggestion that these forms of ego boundary confusion <u>are</u> sufficient to rule out an OCD diagnosis). D.S. was a 29 year old woman whose major fear was that unwanted "obscene" thoughts she had might travel from her head, down her arms and out of her body onto objects she was touching, and that she would be punished for having left these thoughts on these objects. This situation she described as her thoughts "contaminating" the objects. She reported that she often tried not to think or to stop thinking about her thoughts leaving her body (that is, she resisted these thoughts), but was ambivalent as to the morbidity of this way of thinking (that is, her reported insight was at most partial). Her response to thinking that her thoughts had contaminated objects was to repeat (up to 50 times) whatever she had been doing, for example, getting up out of a chair, at the time the contamination was supposed to have taken place. This, she reported, would eventually erase the thought from her mind and she would then think that this thought had also been removed from the object it had been contaminating.

While it might be argued that the diagnosis this woman was given was questionable, how, working on the tentative assumption that it is correct, might one seek to justify it? Why, in particular, should this woman not be diagnosed as being at least partially deluded? Is this not supported by the content of her thinking having the theme it does, while she expresses some (but by no means complete) insight into the morbidity of this thinking?

Against a "partially deluded" diagnosis, is it perhaps the <u>behaviour</u> of this patient - her repetitive action intended to undo harm thought by her to have occurred - which makes her diagnosable as OCD? And if she were to report the very same thinking - a half doubted fear that her thoughts had escaped and were contaminating surrounding objects - in the absence of any repeated action, would the OCD diagnosis not then seem inappropriate, and a "partially deluded" or "deluded" diagnosis far more likely to be correct? On this argument, the patient's symptoms can be distinguished from bizarre delusions (or bizarre partial delusions), but not on the grounds either of the "form" or the "content" of her thinking. Her thinking can thus not

be distinguished from such deluded or partially deluded states at all when considered in isolation from its behavioural consequences, it is being tentatively suggested.

Another example may help to argue the same conclusion. Reed (1985, p141) presents two patients - "Mr E", whose everyday life was severely constricted by his fear about the insidious spread of germs, disease and corruption, and "Mr A", who harboured fears of identical content. The former patient is described by Reed as having an obsession, the latter a delusion of the "nihilistic" variety as is not uncommonly observed in depressive disorders. Reed suggests that the fact "Mr E" resists his thoughts is crucial to this distinction, but this cannot be so because, as has been above (see section 1.2.6.2), some OCD patients do not resist obsessions which are otherwise very like those this patient reports.

Among other grounds on which such a nihilistic delusion might instead be distinguished from obsessions with similar contents is arguably once again the behavioural consequences of the patient's thinking. Thus, some depressed patients might say that they regard the world as full of disease and contamination, but it is only when a patient responds to this thinking by resorting to soap and scrubbing brushes etc. to protect himself and/or others, that it is perhaps at least more likely that an alternative or additional diagnosis will be OCD.

It must once again be stressed that these tentative suggestions amount to no more than the claim that <u>some</u> obsessions may be distinguished from delusions by the behaviour which they provoke. The necessity for this restriction to only "some obsessions" is most obviously argued by the fact that in certain cases obsessions may provoke no compulsive behaviour at all, and could obviously by virtue of this scarcely be either characterised as obsessions or distinguished from delusions. Other criteria must be at work in these cases, with such features as the involvement of hairsplitting doubts, and/or endless speculation of "what if..." variety, perhaps sometimes playing a role in characterising the patient's thinking as OCD.

A similar picture to that observed in the case of the phobia/OCD distinction already begins to emerge here, therefore. There is no one way, or single dimension in terms of which, delusions and obsessions may be distinguished. There are different ways, with features, such as repetitive behaviour, which are not common to all cases of OCD, once again at least sometimes playing a part in distinguishing some obsessions from delusions.

1.3.11 Summary

The discussion suggests that there is no single way in which OCD may be distinguished from phobic or delusional states and a number of different ways in which these distinctions may be drawn have been noted. In the next chapter, the results of empirical

investigations of the nature of obsessive-compulsive experience in OCD patients will be reported. This will return the discussion to the standard diagnostic criteria, and the work of Stern and Cobb (1978), discussed above (see section 1.2).

Chapter two: Characteristics of obsessive-compulsive experience - two empirical studies

2.1 The first study: a principal components analysis of some of the characteristics of obsessive-compulsive experience

2.1.1 Stern and Cobb on senselessness, resistance and reassurance

A number of questions concerning the characteristics of obsessive-compulsive experience follow directly from the discussion of the definition of OCD which is offered above (chapter 1). According to that discussion, not all OCD symptoms are experienced as senseless or absurd by their sufferers or are resisted by them. What proportion, then, of clinical obsessions and compulsions do exhibit these features and - given that these features may be present to a greater or lesser degree (Reed 1985) - to what extent do those obsessions and compulsions which exhibit them do so? How many tend to be strongly resisted and/or experienced as completely senseless and how many exhibit these features only to a more moderate degree?

Stern and Cobb (1978) also examined these questions and found, using an observer-rated five point scale with a sample of 45 OCD patients, that "78 per cent of patients rated their rituals as either "rather silly" or "absurd" [that is, reported a moderate to maximum degree of perceived senselessness as regards their rituals] whereas only 54 per cent showed moderate to maximum resistance" (p238).

Stern and Cobb also examined a number or other questions, including the extent to which patients were provided with, and the extent to which their rituals were reduced by, the reassurance of others. Reassurance occurred in a moderate to extreme degree in 47% of cases and had only moderate to no effects in 73%.

2.1.2 The multidimensionality of obsessions and compulsions

The present investigation also raises the question of whether or not obsessions and compulsions are "multidimensional phenomena" (Kendler et al 1983) with respect to certain of their features. This question, although related to those mentioned above, has been directly inspired more by the literature on the characteristics of delusional experience than it was by that on the nature of obsessions and compulsions, and a brief outline of some of the former literature is therefore included in what follows as background to the present investigation.

A clinical phenomenon's "multidimensionality" is understood here to consist of that phenomenon's exhibiting two or more features which vary, but do not co-vary, across the different patients in whom that phenomenon may be observed. The term "multidimensional" is in fact a misnomer in that it is not necessary for the features in question to appear as dimensions for the phenomenon which exhibits them to be "multidimensional", as this term

is being used here - a phenomenon would be classified as such by virtue of there being a low correlation across different patients of two or more of its all-or-none features. It is perhaps also worth emphasising that it is more useful to consider whether or not any clinical phenomenon is multidimensional with respect to this or that set of features rather than simply multidimensional per se. It seems likely that for any clinical phenomenon there will be bound to be some features with respect to which it is multidimensional.

The question of a clinical phenomenon's multidimensionality must be distinguished from the question of whether or not there is a continuum between that phenomenon on the one hand and normal experience and behaviour on the other. Thus, the suggestion that delusions are a multidimensional phenomena with respect to such features as, for example, the amount of insight patients exhibit and the preoccupation and distress patients report, is perfectly consistent with the claim that delusions are qualitatively distinct from normal thinking because of, for example, the nature of their contents and/or the grounds on which they are believed.

2.1.3 Strauss and Kendler et al

Consider, then, the literature on the characteristics of delusional experience which forms the background to some of the present investigation. Strauss (1969) argued that both hallucinations and delusions should be thought of as "points on continua function" due to "the difficulty in determining presence or absence of [these] phenomena in the many patients who present intermediate kinds of experiences" (p581). Of relevance here is the distinction discussed above between on the one hand whether or not a clinical phenomenon is multidimensional and on the other whether or not it is qualitatively different from normal experience; it is the latter question which is the central concern of Strauss's investigation. This question would appear to hold more interest in the case of delusions than in that of obsessions and compulsions, most of which are evidently not qualitatively distinct from normality (de Silva 1988). A number of Strauss's observations, furthermore, concern merely empirical difficulties which may arise in attempting to determine whether or not a given phenomenon is a delusion or hallucination, rather than conceptual problems which are encountered in attempting to distinguish these phenomena from normal thinking and experience. Strauss's paper nonetheless does identify a number of different ways in which conceptual difficulties may arise for an all-or-none distinction between delusional and normal thinking and this appears to have helped inspire Kendler et al's (1983) interest in the multidimensionality of delusions - as these authors themselves acknowledge, their dimensions [i] and [iii] (see below) are taken from Strauss's paper.

Attempting to show that delusions are "a multidimensional clinical phenomena"

(p446), Kendler et al identified five dimensions of delusional experience and investigated the extent to which these dimensions correlated with one another. The dimensions of delusional experience they examined in observer-rated interviews were: "[i] Conviction - the degree to which the patient is convinced of the reality of the delusional beliefs...[ii] Extension - the degree to which the delusional belief involves various areas of the patient's life...[iii] Bizarreness - the degree to which the delusional belief departs from culturally determined consensual reality...[iv] Disorganisation - the degree to which the delusional beliefs are internally consistent, logical, and systematized...[v] Pressure - the degree to which the patient is preoccupied and concerned with the expressed delusional belief" (1983,p466-7). (Note that the "extension" of delusions must be being defined here in terms of how many areas of the patients life feature in his delusional thinking rather than by how many of these areas involve the patient's functioning being disrupted - the latter definition would fail to keep dimensions [ii] and [v] conceptually distinct.)

In addition to investigating the degree of correlation among their dimensions of delusional experience Kendler et al also examined both (1) the related question of whether or not "factor analysis [could] extract from these five dimensions a small number of factors that could provide an empirical basis for understanding the structure of delusional experience" (it is in fact unclear whether Kendler et al used a factor or principal components analysis) and (2) whether or not their postulated dimensions could be reliably measured by different observers.

Kendler et al found that the correlations between their five dimensions were all fairly low, the highest between any two dimensions being only .36. Factor analysis revealed two factors - the dimensions which loaded highest on one were conviction and pressure (extension also had a reasonably high loading on this factor), those which loaded highest on the other were bizarreness, extension and disorganisation. Kendler et al termed the first of these factors "delusional involvement", the second "delusional construct"; they suggested (p468) that the first factor reflects the cognitive and emotional intensity of the patient's involvement with his delusions and that the second (their suggestion is perhaps rather unclear here) reflects the "manner in which the patient structures the delusional belief" (p468).

Kendler et al pointed out that the low correlations among their various dimensions of delusional experience carry important implications for the assessment of the severity of delusions, and in particular suggest that one may not always be able to speak of one delusional experience being simply more or less severe than another - only of its being so in this or that respect. Kendler et al asked "if a patient's delusions become less bizarre while the patient's

conviction and pressure regarding the delusions increases, has the patient become more or less delusional?" (p468) and cite one study (Hole et al 1979) in which such independent changes in different characteristics were observed. The same point clearly applies to comparisons of the severity of the delusions suffered by different patients - if the delusions of one patient are highly bizarre but accompanied by low conviction and low pressure, while another's exhibit the opposite profile, who is to say which of these patients is the most severely deluded?

Kendler et al also reported that the ratings of four of their postulated dimensions "were reliable with adequate to excellent interobserver reliability" (p468). The one exception was bizarreness, Kendler et al reporting that it was most difficult to achieve agreement as to the rating of this characteristic when the patient's delusions were, or had much in common with, "commonly accepted religious beliefs" - they give the example of a man who "believed that his mother was a virgin when he was born and that strangers consequently regarded him as special".

2.1.4 Garety and Hemsley

Garety and Hemsley (1987) suggested that more characteristics than the five studied by Kendler et al are relevant to delusions and included eleven in their investigation, these being: "[i] conviction, [ii] preoccupation, [iii] interference,...[iv] resistance, [v] the degree to which the belief is dismissable from the mind, [vi] absurdity, [vii] the extent to which the belief is self-evident, [viii] the degree to which reassurance is sought from others, the extent to which the belief causes [ix] worry and [x] unhappiness and...[xi] the pervasiveness of the belief" (p295). The patient's degree of "resistance" to his delusional thoughts was defined by Garety and Hemsley in terms of the extent to which the patient did not like to have them and the degree to which such thoughts were "self-evident" was defined by them in terms of the extent to which the patient found them to be "completely obvious" (high self evidence) or "utterly strange, implausible" (low self evidence). The degree of "interference" produced by delusions was defined in terms of the extent to which they made a difference to what patients did, while their degree of "pervasiveness" was defined in terms of the extent to which the patient was able to think about other things at the same time as thinking about them.

These characteristics were, in contrast to those included in Kendler et al's study, self-rated, that is assessed by the deluded individual rather than by an observer. As Garety and Hemsley note, this form of assessment meant that bizarreness could not be rated, it being "a characteristic which can only be observed and not self rated" (p297) - a patient's judgement as to how bizarre his own delusion is seems if anything most likely to reflect the quite different characteristic of his degree of conviction as to the truth of the delusion rather than

its bizarreness. The same would appear to apply to self-ratings of "absurdity" and "strangeness or implausibility", both of which are included in Garety and Hemsley's study and would arguably both amount to ratings of bizarreness if assessed by an observer.

Neither of Kendler et al's dimensions of "disorganisation" and "extension" is included in Garety and Hemsley's study and although Garety and Hemsley do not discuss this, it seems reasonable to suppose that disorganisation is, like bizarreness, a characteristic which would be difficult to assess by self-ratings. It seems reasonable to suppose, that is, that deluded patients will often be poor judges of how consistent and logical their delusions are. There appears, by contrast, to be no good reason for the exclusion of the characteristic of extension from Garety and Hemsley's investigation.

It is, then, only the two dimensions from Kendler et al's first factor - "conviction" and "pressure" - which are included in Garety and Hemsley's investigation. How do these two characteristics relate to the eleven studied by Garety and Hemsley?

In order to answer this question, consider first a subdivision of these eleven characteristics on semantic grounds. Characteristics [i] (conviction), [vi] (absurdity) and [vii] (self-evidentness) all appear to involve how much <u>insight</u> the patient has into the pathological nature of his thinking (the inclusion of characteristic [vii] in this category might be objected to on semantic grounds, but the findings of both the present investigation [see section 2.1.7] and Garety and Hemsley's support its inclusion). Characteristics [ii] (preoccupation), [iii] (interference), [v] (difficulty of dismissal - termed "dismissability" in Garety and Hemsley's investigation) and [xi] (pervasiveness) might all be described as involving the <u>prominence</u> of the patient's pathological thinking in his mental life. Characteristics [iv] (resistance - see above for its definition), [ix] (worry) and [x] (unhappiness) might all be described as dealing with the <u>upset</u> which the patient is caused by this thinking, while characteristic [viii] (reassurance-seeking) would appear to stand on its own. A principal components analysis reported by Garety and Hemsley (1987) produced four components which corresponded to this semantic sub-division of their scale, except for characteristics [iii] and [xi] failing to load on any component (p296-7).

Returning then to the relationship between these eleven characteristics and Kendler et al's, it seems reasonable to suppose that Kendler et al's dimension of "pressure" involves Garety and Hemsley's characteristics [ii], [iii], [iv], [v], [ix], [x] and [xi], that is all of the characteristics which were included above in the "prominence" and "upset" categories. (Garety and Hemsley themselves suggest that Kendler et al's study has no measure of upset but this is surely to overlook the inclusion by Kendler et al in their definition of "pressure" the

patient's degree of concern with his delusional belief). It also seems reasonable to suppose that Kendler et al's dimension of "conviction" corresponds to Garety and Hemsley's characteristics [i], [vi] and [vii], that is those characteristics included above in the "insight" category (these characteristics would also seem to correspond to Stern and Cobb's "absurdity" scale discussed earlier). Only characteristic [viii] (reassurance-seeking) from Garety and Hemsley's study, then, appears not to be related to the dimensions studied by Kendler et al. In sum, Kendler et al's two dimensions of "conviction" and "pressure" may be said to succeed in covering the same ground as all but one of Garety and Hemsley's eleven characteristics, while Garety and Hemsley's principal components analysis shows that the features of concern (as measured by their characteristics [iv], [ix] [xi]) and preoccupation (as measured by their characteristics [ii] and [v]) are independent of one another, contrary to Kendler et al's inclusion of both of these features under the single dimension of "pressure".

Three further points are worth noting. Firstly, Kendler et al reported only the extent to which scores on each of their dimensions related to one another, not what proportion of their patients scored at the various levels for each of these dimensions. Garety and Hemsley (1987), by contrast, do provide such information and report a considerable degree of inter-subject variability on many of their characteristics (see below for the details of these findings). Secondly, while the question of inter-rater reliability can clearly not arise for Garety and Hemsley's rating scale, the absence of evidence supporting the accuracy of self-ratings of the various characteristics included emphasises the necessity to regard Garety and Hemsley's findings as preliminary. The same applies to the findings of the present investigation in which Garety and Hemsley's rating form was used (see below). Comparisons between the findings of the present investigation and those reported by Stern and Cobb are hampered by the similarly preliminary nature of both their observer-rated inquiry and the present one. Thirdly, the present findings, like those of Stern and Cobb and Garety and Hemsley, do not enable one to determine how stable the patients' ratings would be over time and it is thus more accurate to describe the level of the features recorded as characterising the patient's response rather than the patient. Similarly, no attempt is made in the present investigation to determine whether or not systematic or predictable variations in ratings occur, for example no evidence is presented as to whether patients report less insight when they are in the situations in which they carry out their compulsive behaviour.

2.1.5 The present study

The rating form used by Garety and Hemsley (1987) with deluded subjects was employed here with the present sample of OCD patients. What, then, are the questions which

this rating form may be used to answer as regards the experience of this sample of patients? And what predictions is it possible to make as to what these answers are likely to be? The questions and predictions examined in the present investigation were are as follows:

(a) How many of these patients report their obsessions and compulsions to be experienced as senseless, as measured by items [i] (conviction), [vi] (absurdity) and [vii] (self-evidentness)? Stern and Cobb's (1978) findings would lead one to predict that the majority of patients will record moderate to high scores on item [vi], and moderate to low scores on items [i] and [vii] - these findings would lead one, that is, to predict that the majority of patients will report a moderate to high degree of insight or perceived senselessness on all of these items.

(b) How many patients report that they resist their obsessions and compulsions? Although Garety and Hemsley's rating scale includes a measure of "resistance" this is, as noted above, couched in terms of how much the patient likes to have the thoughts in question on his mind and is thus of little relevance to the characteristic of resistance which features in the literature on OCD (Garety and Hemsley's characteristic will be referred to as "resistance (1)" in the tables below). Garety and Hemsley's scale was consequently supplemented for the present investigation by two further items: [xii] "resistance (2)", which asks the patient the extent to which he tries to keep his obsessions (and/or compulsive urges) off his mind (that is, tries not to start having them) and [xiii] "resistance (3)", which asks the patient the extent to which he tries to get his obsessions (and/or compulsive urges) off his mind (that is, tries to stop having them). Stern and Cobb's findings would lead one to predict that little more than half of the patients will record moderate to high scores on these scales.

(c) To what extent is reassurance sought by OCD patients and to what extent does it reduce their discomfort? And to what extent is the discomfort of these patients reduced by having someone else carry out the tasks which ordinarily cause them difficulty? Only the extent to which reassurance is sought is covered by Garety and Hemsley's rating scale (characteristic [viii] - this will be referred to as "reassurance (1)" in the tables below) and so their scale was supplemented for the present investigation by a further three questions - [xiv] "reassurance (2)", which asks the patient the extent to which reassurance reduces his discomfort, [xv] "reassurance (3)", which asks the patient the extent to which having asked for reassurance once he needs to ask for it again immediately afterwards and [xvi] "others' action", which asks the patient the extent to which other people's carrying out for him the tasks which cause him difficulty reduces his discomfort more or less than his carrying out these tasks for himself.

All four of these characteristics [viii], [xiv], [xv] and [xvi] deal with the extent to which other people's help is sought by, or is beneficial to, the patient. Stern and Cobb's findings lead one to predict that reassurance will be sought to a moderate or extreme degree by almost half of the patients, insofar as the extent to which reassurance is sought may be inferred from the extent to which reassurance is provided, the characteristic investigated by Stern and Cobb. Stern and Cobb's findings also enable one to predict that reassurance should help to a moderate or extreme degree (that is, moderate to high scores on characteristics [xiv] and [xv]) in less than half of the cases. Insofar as characteristic [xvi] may be expected to correlate with these two characteristics (see below) one may also tentatively predict that less than half of the responses given will indicate the action of other people to be more effective than that of OCD patients themselves in reducing their discomfort.

(d) Are obsessions and compulsions multidimensional phenomena? Will the 16 characteristics introduced above all correlate highly with one another and, if not, which will tend to correlate most highly with which? Less empirical evidence is available as regards these questions, but four predictions seem possible. Firstly, Stern and Cobb's finding that significant levels of resistance are less common than significant levels of reported insight is clearly relevant. This evidently means that one should not have much confidence in reported insight and resistance - that is characteristics [i], [vi] and [vii] on the one hand and [xii] and [xiii] on the other - turning out to be very closely related here.

Secondly, in contrast to delusions it appears that almost all if not all obsessions and compulsions will be given high ratings on those characteristics which involve the degree of prominence of the patients symptoms - that is, [ii],[iii],[v] and [xi] - and that almost all if not all obsessions and compulsions should also receive high ratings as regards those characteristics which involve the degree of upset the patient is caused by his symptoms - that is, [iv], [ix] and [x]. All seven of these characteristics should emerge, therefore, as closely related in a population of OCD patients, it is predicted. It is thus also predicted that the distinction, which was observed in the case of Garety and Hemsley's (1987) deluded subjects, between (some of) the characteristics included in the categories of "prominence" on the one hand and "upset" on the other, should not emerge as regards the ratings of OCD patients. There should therefore also be no necessity as regards the ratings of these patients to subdivide Kendler et al's dimension of "pressure", which was argued earlier to consist of a combination of these categories of "prominence" and "upset".

Thirdly, it seems that one may further predict that none of the seven characteristics involved in the degree of prominence of, or upset caused by, obsessions and compulsions will

emerge as closely related to either the degree of perceived senselessness or resistance OCD patients report as regards their symptoms. This follows from what has already been predicted so far. If clinical obsessions and compulsions are as predicted all prominent in the experience of their sufferers and all provoke high levels of distress, these characteristics of prominence and distress cannot correlate very highly with characteristics such as the degrees of resistance and insight reported, which vary to a considerable extent across different OCD patients.

Fourthly, what of those characteristics which have been described as involving the extent to which other people's help is sought by, or is beneficial to, OCD patients, that is characteristics [viii], [xiv], [xv] and [xvi]? On the basis of this common characterisation of these four features one may very tentatively predict that they will emerge as closely related to one another, although arguably it is difficult to include characteristic [viii] in this prediction - one should perhaps not necessarily expect those patients who are most benefitted by the help of others to seek that help most often. The extent to which these four characteristics will correlate with any of the others appears to be an open question.

It is arguable that none of the above predictions concerning which of the sixteen characteristics in the present investigation should be closely related to one another provide a sufficient basis on which to predict specifically that those characteristics which are so related will form separate components in principal components analyses. Their forming such components would, however, clearly confirm these predictions.

2.1.6 Method

2.1.6.1 Subjects

Patients were recruited from the Psychological Treatment Unit at the Maudsley Hospital, London. Six patients who were approached refused to take part in the investigation. The 55 patients (34 female, 21 male) included had all been diagnosed by a senior psychiatrist as suffering from OCD (DSM III-R 300.30). They had all been ill for at least one year and had reported no psychotic, severe affective or physical illnesses to clinical staff. Those patients who were receiving medication had been on a stable dose for at least 6 months. The type of symptom reported (checking, cleaning or other) was ascertained in an interview with the same experimenter (ICJ). The symptoms of 26 patients consisted mainly or wholly of cleaning difficulties, the symptoms of 23 of them consisted mainly or wholly of checking difficulties. The symptoms of a remaining 6 patients could not be classified in either of these ways, involving such matters as for example wanting other people to move in a certain way (without any apparent fear of contamination or potential misfortune being associated with this),

repeating actions to remove "bad thoughts" - for example, thoughts of doing violence to others - or attempting to form "good" thoughts in order to replace such thoughts etc. 10 of the cleaners were men and 16 women, 8 of the checkers were men and 15 women. The third group contained 3 men and 3 women. All 55 patients were aged between 18 and 65 years of age. The mean age of the whole sample was 39 years (SD=11 years), with the ages of the checkers and cleaners being identical (mean=38 years, SD=11 years). The six patients in the third group tended to be slightly older (mean=42 years, SD=11 years).

2.1.6.2 Measure

Patients were asked to assess their obsessions and compulsions in terms of each of the 16 characteristics on a visual analogue scale, identical to that devised by Garety and Hemsley (1987) save for the addition of five characteristics as discussed above. Each characteristic was represented as a line which had at one end a brief description of the highest possible rating of that characteristic and at the other a brief description of its lowest possible rating. Thus, the line on which the characteristic of conviction was to be rated had at one end the words "believe absolutely" and at the other "believe not at all", while the line on which preoccupation was to be rated had at one end "thinking about it all the time" and at the other "not thinking about it at any of the time, ever" (see figure 2.A for full scale). The subject was required to mark on each line the extent to which his obsessions and compulsions possessed the characteristic in question. As with Garety and Hemsley's scale, the direction in which more or less extreme scores were to be indicated switched between different characteristics to control for response acquiescence. The lines were not subdivided, the score for each characteristic being determined by the distance of the patient's mark from the end of the line which represented the lowest possible rating of the characteristic in question. Raw scores were adjusted to provide ratings of 0-10 for each characteristic, these ratings in turn being divided into low (0-3.3), moderate (between 3.3 and 6.7) and high (6.7-10) scores.

Figure 2.A: The characteristics of obsessive-compulsive experience rating scale

(Direction in which more or less extreme scores were to be indicated varied as shown)

[i] Conviction
Believe absolutely . . . . . Believe not at all

[ii] Preoccupation
Not thinking about it . . . . . Thinking about it
at any time, ever — all the time

[iii] Interference
Makes an enormous difference . . . . . Makes no difference to
to what I do — what I do

[iv] Resistance (1)
Very much like thinking . . . . . Do not like thinking about
about it — it at all

[v] Difficulty of dismissal
Cannot dismiss it at all . . . . . Can dismiss it
from my mind — easily from my mind

[vi] Absurdity
Seems entirely sensible . . . . . Seems entirely senseless

[vii] Self-evident
Seems completely obvious . . . . . Seems completely strange, implausible

[viii] Reassurance (1)
Seek reassurance about . . . . . Do not seek reassurance
it all of the time — about it

[ix] Worry
Thinking about it does not . . . . . Thinking about it makes me
make me worry at all — very worried

[x] Unhappiness
Thinking about it makes . . . . . Thinking about it does not make
me very unhappy — me at all unhappy

[xi] Pervasiveness
Cannot think about other things . . . . . Easy to think about other things
at all when thinking about it — while thinking about it

[xii] Resistance (2)
Try hard to keep it off my mind . . . . . Do not try to keep it off my mind

[xiii] Resistance [3]
Try hard to get it off my mind . . . . . Do not try to get it off my mind

[xiv] Reassurance (2)
Reassurance reduces my . . . . . Reassurance does not reduce
discomfort about it — my discomfort about it

[xv] Reassurance (3)
I do not need to ask for reassurance . . . . . I need to ask for reassurance
more than once — more than once

[xvi] Others' action
Other's action reduces my . . . . . My action reduces my
discomfort more than my action — discomfort more than other's action

2.1.6.3 Procedure

The procedure was the same as that described in Garcty and Hemsley (1987). Patients recorded their ratings during an interview lasting approximately half an hour, during which a consent form was also completed (see Appendix B). The choice of wording for the obsessions and compulsions which were to be rated was agreed with the patient and then entered at the top of the scale. For example, the statement that "many things are contaminated and I may contract and pass on disease through contact with them unless I clean myself and objects a great deal" would be entered at the top of the scale if the patient was satisfied that this was an adequate summary of his obsessions and compulsions. Patients rated each characteristic in the presence of the interviewer (ICJ), items being explained and clarifications offered as required.

2.1.7 Results

The means and standard deviations for each characteristic are given in Table 2.A (columns 1 and 2) as are the percentage of patients who recorded high, moderate and low scores on each characteristic (columns 3 to 5).

Pearson correlation co-efficients among the characteristics were calculated and the resultant matrix is given in Tables 2.B.1 and 2.B.2. A number of principal components analyses (SPSS:PC+ v.3.1 [1988]) were conducted. The first pair of these analyses (see Table 2.C.1 and 2.C.2) were on the original eleven characteristics from Garety and Hemsley's rating scale. The second pair of analyses (see Table 2.D.1 and 2.D.2) were performed on those eleven characteristics plus the two additional ones [xiii] and [xiv] which were designed to measure the patient's degree of resistance. In the case of both of these pairs of analyses, the first analysis (those shown in Table 2.C.1 and 2.D.1) was conducted without a maximum number of components being specified. Such a number was specified for both of the second analyses (those shown in Tables 2.C.2 and 2.D.2), in order to accentuate the findings reported in the first analyses (see below). The maximum number of components specified in the first of these analyses was two, the maximum specified in the second was three. The final principal components analysis reported (Table 2.E) was carried out on all sixteen characteristics included in the present investigation, with a maximum number of four components being specified (this was done to see if the four categories of item identified in predict [d] [see section 2.1.5] would emerge here as separate components, their having failed to do so in a principal components analysis on all sixteen characteristics when no maximum number of components was specified). (The following abbreviations are used on the tables in this chapter: "df. of dismissal" = difficulty of dismissal, "oth. action" = other's action.)

In all of the tables positive loadings less than +0.4 on any component have been excluded for the sake of clarity. The characteristics with loadings equal to or greater than +0.4 on the same component are described in the text as loading together on that component, those with positive loadings smaller than +0.4 on any given component are described as having failed to load on that component. Characteristics are described as loading negatively on a component if their loading is a negative number equal to or greater than -0.4. A number of characteristics fell just short of a +/-0.4 loading on components in a number of these analyses. In the first analysis (Table 2.C.1) characteristic [vii] had a loading of +0.37 on component 1 and characteristic [iii] had a +0.39 loading on component 3. In the third analysis (Table 2.D.1) characteristic [v] had a loading of +0.34 on component 1, characteristic [xii] had a loading of +0.37 on component 2 and characteristic [i] had a loading of -0.38 on component 3. In fifth analysis (Table 2.E) characteristic [vii] had a loading of +0.37, and characteristic [xii] had a loading of +0.30, on component 1.

The ratings of checkers and cleaners did not differ significantly from one another on any of the characteristics examined, save for characteristic [i] on which the scores of cleaners (mean=7.3, SD=2.1) were significantly ($p<.05$) greater (indicating a higher level of conviction) than those of checkers (mean=6.0, SD=3.0). The third group of patients had ratings which were significantly different to those recorded by both checkers and cleaners on <u>all</u> of the characteristics included in the present study.

Table 2.A: The responses of the subjects on the characteristics of obsessive-compulsive experience rating scale

	Total Group Mean	Total Group SD	% S High	% S Mod	% S Low
[i] Conviction	6.4	2.8	63.5	23.5	13
[ii] Preoccupation	7.5	2.4	78	15	7
[iii] Interference	8.9	1.0	95	5	0
[iv] Resistance (1)	8.8	1.5	91	7	2
[v]Df of Dismissal	8.3	1.8	89	9	2
[vi] Absurdity	6.0	3.1	53	23.5	23.5
[vii] Self-evidentness	4.4	3.2	29	31	40
[viii] Reassurance (1)	5.9	3.5	60	9	31
[ix] Worry	8.5	1.5	91	7	2
[x] Unhappiness	8.9	1.1	93	7	0
[xi] Pervasiveness	8.2	1.8	89	9	2
[xii] Resistance (2)	5.4	3.9	56	7	37
[xiii] Resistance (3)	4.8	3.9	45.5	9	45.5
[xiv] Reassurance (2)	4.0	3.5	33	8	59
[xv] Reassurance (3)	2.9	3.5	21.5	8	70.5
[xvi] Oth.Action	4.0	3.9	53	8	39

Table 2.B.1: Correlations between the ratings of subjects on items [i]-[xi] on the characteristics of obsessive-compulsive experience rating scale

	i	ii	iii	iv	v	vi	vii	viii	ix	x	xi
i	1.0	-.13	.19	.03	.10	.59**	.57	.03	-.00	-.03	.17
ii		1.0	.37*	.39*	-.12	-.27	.20	-.29	.38*	-.16	-.17
iii			1.0	-.40	.44**	.08	-.07	.21	-.35	.18	.28
iv				1.0	-.08	.15	.29	-.21	.52	-.43**	-.11
v					1.0	-.03	.02	.21	-.24	.18	.13
vi						1.0	.56**	.07	-.07	-.12	.17
vii							1.0	-.12	.18	-.27	-.09
viii								1.0	-.20	.26	.18
ix									1.0	-.63**	-.26
x										1.0	.25
xi											1.0

1-tailed significance: * =.01 ** =.001

Table 2.B.2: Correlations between the ratings of the subjects on items [xii]-[xvi] and between these items and items [i]-[xi] on the characteristics of obsessive-compulsive experience rating scale

	xii	xiii	xiv	xv	xvi
i	-.14	-.11	-.13	-.06	-.03
ii	.05	.04	-.31	.02	-.14
iii	.07	-.04	-.09	-.03	.06
iv	-.08	-.13	.16	.16	.09
v	-.12	-.16	.05	.04	-.07
vi	-.28	-.31*	-.04	.13	-.07
vii	-.05	-.23	-.24	-.02	-.33
viii	.26	-.11	.30	.36*	.24
ix	-.15	-.20	.005	-.26	.10
x	.26	.31	-.00	.28	.04
xi	.05	-06	-.05	.27	.17
xii	1.0	.42**	.16	.12	-.02
xiii		1.0	.11	-.10	.16
xiv			1.0	-.27	.47**
xv				1.0	-.16
xvi					1.0

1-tailed significance: * =.01 ** =.001

Table 2.C.1: Principal components analysis of the responses of the subjects on items [i]-[xi] on the characteristics of obsessive-compulsive experience rating scale

	Variable	Loading	% Variance
Component 1	[ii] Preoccupation	+0.60	28.1
	[iii] Interference	+0.65	
	[iv] Resistance	+0.71	
	[v] Df. of Dismissal	+0.42	
	[viii]Reassurance(1)	+0.48	
	[ix] Worry	+0.78	
	[x] Unhappiness	+0.68	
	[xi] Pervasiveness	+0.44	
Component 2	[i] Conviction	-0.85	20.7
	[vi] Absurdity	+0.85	
	[vii] Self-evidentness	-0.76	
Component 3	[v] Df of Dismissal	+0.80	9.8

Table 2.C.2: Principal components analysis of the responses of the subjects on items [i]-[xi] on the characteristics of obsessive-compulsive experience rating scale (max. of two components specified)

	Variable	Loading
Component 1	[ii] Preoccupation	+0.63
	[iii] Interference	+0.67
	[iv] Resistance	+0.67
	[v] Df. of Dismissal	+0.44
	[viii]Reassurance(1)	+0.49
	[ix] Worry	+0.77
	[x] Unhappiness	+0.64
	[xi] Pervasiveness	+0.47
Component 2	[i] Conviction	-0.83
	[vi] Absurdity	+0.84
	[vii] Self-evidentness	-0.81

Table 2.D.1: Principal components analysis of the responses of the subjects on items [i]-[xiii] on the characteristics of obsessive-compulsive experience rating scale

	Variable	Loading	% Variance
Component 1	[ii] Preoccupation	+0.53	24.5
	[iii] Interference	+0.58	
	[iv] Resistance(1)	+0.71	
	[vii] Self-evidentness	-0.46	
	[viii]Reassurance(1)		
	[ix] Worry	+0.46	
	[x] Unhappiness	+0.77	
	[xi] Pervasiveness	+0.72	
		+0.40	
Component 2	[i] Conviction	-0.76	19.3
	[vi] Absurdity	+0.83	
	[vii] Self-evidentness	-0.60	
	[xiii] Resistance (3)		
		+0.49	
Component 3	[vii] Self-evidentness	-0.41	10.6
	[xii] Resistance (2)		
	[xiii] Resistance (3)	+0.58	
		+0.56	
Component 4	[v] Df of dismissal	+0.57	8.3
	[viii] Reassurance	+0.41	

Table 2.D.2: Principal components analysis of the responses of the subjects on items [i]-[xiii] on the characteristics of obsessive-compulsive experience rating scale (max. of three components specified)

	Variable	Loading
Component 1	[ii] Preoccupation	+0.65
	[iii] Interference	+0.70
	[iv] Resistance	+0.66
	[v] Df. of Dismissal	+0.50
	[viii]Reassurance(1)	+0.49
	[ix] Worry	+0.73
	[x] Unhappiness	+0.58
	[xi] Pervasiveness	+0.44
Component 2	[i] Conviction	-0.84
	[vi] Absurdity	+0.82
	[vii] Self-evidentness	-0.81
Component 3	[x] Unhappiness	+0.50
	[xii] Resistance (2)	+0.75
	[xiii] Resistance (3)	+0.78

Table 2.E: Principal components analysis of the responses of the subjects on items [i]-[xvi] on the characteristics of obsessive-compulsive experience rating scale (max of 4 components specified)

	Variable	Loading
Component 1	[ii] Preoccupation	+0.45
	[iii] Interference	+0.82
	[iv] Resistance	+0.74
	[v] Df. of Dismissal	-0.60
	[ix] Worry	-0.67
Component 2	[i] Conviction	-0.75
	[vi] Absurdity	+0.83
	[vii] Self-evidentness	+0.55
	[x] Unhappiness	
	[xii] Resistance (2)	-0.40
	[xiii] Resistance (3)	+0.58
		+0.60
Component 3	[ii] Preoccupation	+0.56
	[vii] Self-evidentness	-0.50
	[xiv]Reassurance(2)	
	[xvi]Oth. Action	+0.79
		+0.76
Component 4	[viii]Reassurance(1)	+0.66
	[x] Unhappiness	+0.52
	[xi] Pervasiveness	+0.50
	[xv]Reassurance(3)	+0.77

2.1.8 Discussion

The great majority of patients in the present sample unsurprisingly recorded high scores on characteristics [ii] (preoccupation), [iii] (interference), [iv] (resistance {1}), [v] (difficulty of dismissal), [ix] (worry), [x] (unhappiness) and [xi] (pervasiveness). On all but one of these characteristics, fewer than 10% of the sample recorded moderate scores and fewer than 3% recorded low scores. The exception was characteristic [ii] (preoccupation), and even here only 15% of patients reported moderate scores and only 7% low. Scores were distributed more widely on the other characteristics. Relatively large numbers of patients reported either high or low scores for characteristics [viii] (reassurance [1]), [xii] (resistance {2}), [xiii] (resistance {3}), [xiv] (reassurance {2}) and [xvi] (other's action), less than 10% of patients recording moderate scores on any of these characteristics. Fewer than 10% of patients once again recorded moderate scores on characteristic [xv] (reassurance {3}), but here more than three times as many subjects recorded high as recorded low scores. Scores were more evenly spread across all three levels for characteristics [i] (conviction), [vi] (absurdity) and [vii] (self-evidentness). In sum, then, the patients in the present study were as predicted most similar to one another as regards the characteristics which involved the prominence of, and upset caused by, their obsessions and compulsions and differed from one another most as regards those characteristics which involved their degree of insight into, and resistance to, these symptoms. They also differed widely from one another on most of those characteristics which measured the degree to which other people's help is sought and is beneficial.

Comparing these scores with those of Garety and Hemsley's deluded patients on the eleven characteristics common to that study and the present investigation, the profile of scores produced by Garety and Hemsley's subjects turns out to be almost precisely the reverse of that produced by the patients in the present study on the majority of these characteristics (one should note here that Garety and Hemsley sub-divide the ratings of their subjects differently - they score ratings from 1-3 as low, 4-7 as moderate and 8-10 as high). Garety and Hemsley's patients (1987, table 1, p296) differ most from one another on characteristics [ii] (preoccupation), [iii] (interference), [v] (difficulty of dismissal), [ix] (worry), [x] (unhappiness) and [xi] (pervasiveness) and are most similar to one another on characteristics [i] (conviction) and [vii] (self-evidentness), the majority recording high levels on these latter two characteristics. As regards the remaining three characteristics the majority of Garety and Hemsley's patients have high scores on [iv] (resistance 1) and low scores on [vi] (absurdity) and [viii] (reassurance 1). These majorities are, however, considerably smaller than those observed among these patients on characteristics [i] (conviction) and [vii] (self-evidentness)

and their ratings on these three remaining characteristics also differ considerably from those of the patients in the present sample. Thus, 91% of the present sample as opposed to only 69% of Garety and Hemsley's recorded high scores on characteristic [iv] (resistance {1}) while 60% of the present sample as opposed to only 21% of Garety and Hemsley's did so on characteristic [viii] (reassurance {1}). 64% of Garety and Hemsley's subjects as opposed to only 23.5% of the present sample recorded low ratings on characteristic [vi] (absurdity). To sum up, Garety and Hemsley's deluded subjects tended, in contrast to the present sample of OCD patients, to differ from one another most on those characteristics measuring the prominence of, and upset caused by, their pathological thinking and to be most similar to one another on those characteristics which measured their degree of insight into that thinking.

Consider now each of the predictions (a)-(d) made earlier in the light of the findings with the present sample of patients.

(a) All three of the measures of perceived senselessness, characteristics [i], [vi] and [vii] - and especially [vi] and [vii] - showed relatively large numbers of patients reporting high, moderate and low scores. On both the absurdity and self-evidentness scales more patients recorded scores indicating high than moderate or low levels of perceived senselessness - 53% recorded high, and 23.5% moderate, levels of absurdity while 40% recorded low, and 31% moderate, levels of self-evidentness. On the conviction measure only 13% of patients in the present sample reported high levels of perceived senselessness (that is, low conviction) and 63.5% reported low levels of perceived senselessness (that is, high conviction). 23.5% recorded moderate ratings on this characteristic. Between 70% and 80% of responses on characteristics [vi] and [vii], therefore, indicate moderate to high levels of perceived senselessness and thus replicate as predicted the levels of absurdity reported by Stern and Cobb's sample. Results from characteristic [i] are markedly different, over three quarters of responses indicating low to moderate levels of perceived senselessness. It is not clear why there should be this difference between these ratings on characteristic [i] on the one hand and on the other the ratings on characteristic [vi] and [vii] and those reported by Stern and Cobb.

(b) 56% of patients recorded high levels of resistance on characteristic [xii] and 45.5% did so on characteristic [xiii]; 37% recorded low scores on characteristic [xii] and 45.5% did so on characteristic [xiii]. 7% reported moderate scores on characteristic [xii], 9% reported such scores on characteristic [xiii]. Moderate to high levels of resistance thus characterise between 50% and 60% of all responses on these two characteristics. This finding therefore replicates as predicted the levels of resistance reported by Stern and Cobb's sample.

(c) Not all of the present sample were able to answer three of the four questions

dealing with the extent to which they were benefitted by the help of other people. This was so for six patients as regards characteristic [xvi]. These six patients either complained of no compulsive behaviour at all or alternatively their compulsive behaviour involved a task which by its very nature could not be attempted by other people, for example forming a "good thought" after experiencing a "bad thought". The question of being helped by other people's action did not therefore arise for these patients.

The majority (60%) of responses on characteristic [viii] indicated high levels of reassurance-seeking, although a substantial minority (31%) recorded low levels. The majority of responses on both characteristic [xiv] (59%) and [xv] (70.5%) recorded low ratings, indicating respectively that a high degree of discomfort-reduction was produced by the reassurance of others, but that having asked for reassurance once patients usually needed to ask for it again immediately afterwards.

These results indicate reassurance-seeking to be slightly more common among the present sample of patients than the occurrence of reassurance among Stern and Cobb's patients. The present finding that the majority of patients have their discomfort reduced by the reassurance of others may at first seem to conflict with Stern and Cobb's findings that reassurance had only moderate to no effects on rituals in nearly three quarters of their patients, but the fact that nearly three quarters of the present sample also indicate that any discomfort reduction they receive from reassurance is only transient suggests that the findings of the two studies may be compatible.

The ratings of the present sample on characteristic (xvi) indicate that, as tentatively predicted, a majority (61%) are not helped more by the action of other people than they are by their own action as regards the tasks which cause them difficulty. A substantial majority (39%) of responses, however, indicate the opposite profile.

(d) The various principal components analyses reported provide some support for the predictions earlier as to which dimensions of obsessive-compulsive experience would emerge as related to one another. In the first analysis (see table 2.C.1), all of the characteristics which involved the degree of prominence of, and upset caused by, the patients' symptoms featured together in the first component. Characteristic [viii] also featured in this component but none of the three characteristics involving perceived senselessness did so, all three of these appearing instead in the second component. A third component in which only characteristic [v] featured also unexpectedly emerged. A varimax rotation produced three components with very similar loadings. These results, then, confirm the predictions that (i) the characteristics involving the degree of prominence of, and upset caused by, the patients obsessions and

compulsions should be related to one another, (ii) characteristics which involve the degree of perceived senselessness patients report should be related to one another and (iii) these two groups of characteristics should be relatively independent of one another. As can be seen in Table 2.C.2, the second analysis, conducted with the same eleven characteristics and in which a maximum of two components was specified, produced largely similar results.

The results of the principal components analysis shown in table 2.D.1, for which characteristics [xii] and [xiii] were added to the eleven from Garety and Hemsley's investigation, preserved the two components from both of the first two analyses except that in this third analysis characteristic [v] just failed to feature in the first component and characteristic [vii] achieved a large enough negative loading to feature in the first component (it also fell only just short of this in the first analysis). Only partial confirmation was provided for the prediction that characteristics [xii] and [xiii] would not be related to the characteristics which feature in either of the two components identified by both the earlier analyses. Although neither of the characteristics [xii] and [xiii] features in component 1, characteristic [xiii] features with characteristic [i], [vi] and [vii] in component 2 while characteristic [xii] only just failed to feature on this component. Similarly, while both characteristics [xii] and [xiii] load together on component 3, characteristic [vii] had a negative loading on the same component while characteristic [i] had a negative loading which was almost large enough for it to feature. A varimax rotation produced reasonably similar results.

The analysis shown in table 2.D.2 removed most of these difficulties for the predictions regarding characteristics [xii] and [xiii]. Components 1 and 2 in this analysis are the same as those in the analyses reported in tables 2.C.1 and 2.C.2 and no other characteristics achieved loadings which were anything like large enough to feature in either of these components. Characteristics [xii] and [xiii] loaded on component 3 together with characteristic [x], the appearance of this third characteristic in this component thus being the only finding in the present analysis which fails to support the predictions made earlier.

The appearance of characteristic [viii] in component 1 in all of these first four analyses neither supports nor fails to support any of the predictions made earlier. It was tentatively predicted, however, that this characteristic will be related to characteristics [xiv], [xv] and [xvi], which were added to the other thirteen characteristics for the next analysis. The results of this analysis are not reproduced here. Six components emerged, all but one failing to resemble any of those from the first four analyses (a varimax rotation produced a similar picture, making use again of six components). A further analysis was carried out on the same sixteen characteristics with a maximum of four factors being specified in the hope that this

might help reproduce the three components identified by the earlier analyses as well as leaving room for a possible fourth component to feature characteristics [viii], [xiv],[xv] and [xvi]. The results, reported in table 2.E, did not fulfil this hope but were more consistent with the earlier four analyses. Component 2 in this last analysis brought together the characteristics which featured in components 2 and 3 in the analysis from table 2.D.2. Component 1 featured five characteristics from the first component of the analyses reported in tables 2.C.1, 2.C.2 and 2.D.2, the three absent characteristics being [viii], [x] and [xi]. All of these three appeared in component 4 of the present analysis, along with characteristic [xv]. Characteristics [xiv] and [xvi] appeared together in component 3, along with characteristics [ii] and [vii]. (These four components together account for 57.5% of the variance in this analysis, a similar figure to that accounted for by, for example, the four components generated in the analysis reported in table 2.D.1.) In this last analysis, therefore, those characteristics which have to do with the extent to which the help of others is sought by and is beneficial to patients emerge as to some degree independent of the components generated by the earlier analyses, to the extent that those components may themselves be said to have been reproduced here. One should, however, emphasise both the distortion of these components in the present analysis and the loading of some of the characteristics from them on the same components as characteristics [viii], [xiv], [xv] and [xvi]. The prediction that these four characteristics would emerge as related to one another is not confirmed by the present analysis, and the absence of any explanation for the division of these four characteristics - two loading on component 3 and two on component 4 - needs to be emphasised.

2.1.9 Summary

The present investigation largely replicates a number of Stern and Cobb's findings, most importantly those concerning the proportion of patients reporting moderate to high levels of perceived senselessness and resistance. This reasonable degree of agreement between Stern and Cobb's findings and the present investigation is especially noteworthy given the use of observer ratings by Stern and Cobb and the use of self-ratings here, although this agreement does of course not provide strong confirmation of the accuracy of the ratings in either study. A reasonably large proportion of responses in the present investigation, like those reported by Stern and Cobb, record low levels of both resistance and perceived senselessness - over 40% of responses are so characterised in the present investigation in the case of resistance and about 25% in the case of two of the three characteristics which involve perceived senselessness; a surprising finding is that on the third characteristic low levels of perceived senselessness were recorded by over 60% of responses.

The present investigation also suggests obsessions and compulsions to be multidimensional phenomena with respect to the characteristics studied. While most obsessions and compulsions were unsurprisingly reported to be upsetting and prominent in the experience of their sufferers, other characteristics both (a) varied considerably across different patients and (b) did so to a considerable extent independently of one another. Does this, then, imply that it may not be possible to compare the obsessions and compulsions of different patients as simply more or less severe than one another, as Kendler et al's findings suggested, as noted earlier, it may not be possible to compare in such terms the delusions of different patients? The present findings may imply this. Variations in the extent to which different patients perceive their obsessions and compulsions as senseless, for example, might be argued to be one way in which the severity of these symptoms varies across different patients. Yet these variations are, on the present findings, independent of the degree of upset provoked by, and degree of prominence of, a patient's obsessions and compulsions. The degree of a patient's insight on the one hand, and the degree of prominence of and upset caused by the patient's obsessions and compulsions on the other, might thus be argued to be independent ways in which the severity of that patient's symptoms may be judged. Against this view, however, one might argue that the extent to which a patient perceives his obsessions and compulsions to be senseless does not determine how severe these symptoms may be judged to be. Although Kendler et al argued with some plausibility that the patient's degree of perceived senselessness is one way in which the severity of his delusional state may be gauged, perhaps this will simply turn out to be a difference between delusions on the one hand and obsessions and compulsions on the other? This claim may be supported by the following point. The extent to which an OCD patient perceives his obsessions as senseless does not determine how obsessed he may be said to be - one may, that is, be completely obsessed by a thought one regards as entirely senseless. This evidently does not hold true in the case of delusions. A deluded patient who comes to perceive his delusion as more senseless may be readily described as having become less deluded.

Having established these various relationships between the characteristics rated by the subjects in the present investigation, these same data were then examined by means of cluster analysis techniques to establish whether they divided patients into distinct groups. The results of this examination are reported in the next section.

2.2 The second study: cluster analyses of patients suffering from OCD

2.2.1 Introduction

Cluster analysis techniques are used to discover whether or not a set of multivariate

data contain distinct groups or clusters of individuals. The present investigation reports the results of a number of such analyses, which were carried out using the same ratings by the same sample of 55 OCD patients as were employed in the principal components analyses presented above (section 2.1). The cluster analyses carried out in the present investigation are to be regarded as purely exploratory exercises, not having been suggested by any hypotheses as to how any subgroups within the present sample of patients might differ from one another with respect to the characteristics examined.

Garety et al (1988) also carried out cluster analyses using eleven of the characteristics included here (characteristics [i]-[xi]) as rated by (and adjusted for) Garety and Hemsley's (1987) deluded patients. As Garety et al's study succeeded in subdividing these patients on the basis of these scores, their findings will be worth bearing in mind when assessing those of the present investigation. Of the solutions Garety et al examined, they chose that which divided their subjects into three clusters containing 16, 18 and 21 members, arguing that this solution "appeared to have most clinical relevance" (1988, p113). They were thus able to describe the three groups identified by their solution as "hebephrenic", "paranoid" and "depressed". While stressing the preliminary nature of their findings, these authors also speculated as to possible differences in aetiology and treatment outcome which might be associated with these different groups.

Differences of five points or more between two of these three groups were observed as regards characteristics [iv] (resistance 1) and [ix] (worry), and differences of the same size emerged between one group and both of the others on two further characteristics, [vi] (absurdity) and [x] (unhappiness) - the "paranoid" group differed from both of the others to this extent on characteristic [vi] while the "hebephrenic" group did so on characteristic [x]. As a result, all three of Garety et al's groups could be distinguished from at least one of the other two groups by between five and seven points on three characteristics, these being [vi], [ix] and [x] in the case of the "hebephrenic" and "depressed" groups and [iv], [vi] and [x] in the case of the "paranoid" group. Differences of between 2.2 and 3.9 points emerged between any two of the three groups on all of the remaining characteristics with the exception of characteristic [i] (conviction), where no difference greater than 0.8 points emerged between any two groups. (Garety et al scored the ratings of their patients from 1-10, as opposed to the 0-10 scoring adopted by the present investigation, introducing a slight artificial reduction in the differences reported in Garety et al's investigation as compared to those reported in the present one.)

2.2.2 Method

For a full account of the materials, procedures and patients used in collecting the data

for the present investigation are provided in the study reported above (see section 2.1). "Single link", "complete linkage" and "group average" cluster analyses were carried out with the ratings of the present sample of patients. All three of these methods are "agglomerative" hierarchical clustering techniques (Everitt 1980), using measurements of similarity and distance between individuals in terms of the characteristics studied to collect those individuals into an ever-decreasing number of groups (see Everitt 1980, ch. 3 for a discussion of the measures of similarity and distance between individuals used by clustering techniques). At any particular stage, all agglomerative hierarchical clustering techniques fuse those individuals and/or groups of individuals they define as closest or most similar to one another (see below) and will ultimately include all of their individuals in a single group. It is for the investigator to determine, therefore, the most appropriate point at which to halt the analysis.

Agglomerative hierarchical clustering techniques offer differing definitions of distance between any two groups (and between any individual and group). The "single link" method defines the distance between any two groups as that between their closest pair of individuals (and the distance between an individual and a group as that between the individual and the group member which is closest to it). The "complete linkage" method adopts the opposite approach, defining the distance between two groups as that between their most remote pair of individuals (and the distance between an individual and a group as that between the individual and the group member which is most remote from it). The definition of distance between two groups adopted by the "group average" method is largely self explanatory, being the average of the distances between all pairs of individuals in the two groups. (The distance between an individual and a group is similarly defined by this method as the average distance between the individual and each of the group members). (See Everitt 1980 for a full discussion of these techniques.) Garety et al's study used a combination of methods - another agglomerative hierarchical clustering technique, "Ward's method", to provide initial estimates of means and variances etc., and the "Normix method" devised by Wolf (1970). These techniques were unavailable for the present investigation.

Cluster analyses were first of all conducted using the ratings of the present sample of patients on the same eleven characteristics as those included in Garety et al's study; further analyses were then performed with the addition of characteristics [xii] (resistance 2) and [xiii] (resistance 3). Characteristics [xiv], [xv] and [xvi] were not included due to the presence of missing values on each of these characteristics. Solutions involving two, three, four and five groups were examined for all three types of analysis.

2.2.3 Results

2.2.3.1 Introduction

The "complete linkage" solutions for three groups using characteristics [i]-[xi], and for three groups using characteristics [i]-[xiii], are shown in Tables 2.F.1 and 2.F.2 respectively. These solutions were not chosen on the basis of their "clinical relevance", in contrast to the solution reported in Garety et al's investigation. Most of the solutions for the present sample of patients produced only one cluster containing more than two or three members, and it is clear that an analysis which so divides a sample of patients provides an at best weak basis on which to propose a sub-division of these patients - clusters would need to contain more members than this for each to sustain such a proposal. These considerations, then, provided the basis on which the solutions seen in Tables 2.F.1 and 2.F.2 were chosen - these were the solutions which contained the fewest groups with only one or two members, and/or the most groups containing a larger number of members.

2.2.3.2 The solution for characteristics [i]-[xi]

The solution shown in Table 2.F.1, which yielded clusters of 15, 5 and 35 members (respectively 27%, 9% and 64% of the sample) was chosen simply because it was that which, using characteristics [i]-[xi] produced the most groups containing at least five patients each while also producing no groups which contained fewer patients than this. Each of the "single link" and "group average" analyses using characteristics [i]-[xi] had only one group which contained more than a single member, while the "complete linkage" solutions for four and five groups using these characteristics only removed one or two patients from the clusters shown in Table 2.F.1 to form additional groups of one or two members each. The "complete linkage" solution for two groups using characteristics [i]-[xi] by definition yielded fewer groups than those shown in Table 2.F.1, doing so by amalgamating clusters one and three from Table 2.F.1 into a group of 50 patients and leaving the five patients from group two as the second cluster.

It can be seen that the mean scores of the three groups shown in Table 2.F.1 are quite similar. Thus, a difference of more than four points between any two group means was observed on only two characteristics, [vi] (absurdity) (where a difference of 4.1 points was observed between groups one [SD=2.4] and two [SD=2.8]) and [x] (unhappiness) (where a difference of 5.3 points was observed between groups one [SD=0.5] and three [SD=1.0]). Differences of between 2 and 3.9 points between any two of the three groups means emerge on all but one of the other characteristics, the exception being characteristic [i] (conviction) where none of the differences between the three groups are as great as 1 point (SD group one=3.3, SD group two=2.5, SD group three=2.9).

There are some similarities between these findings and those reported by Garety et al. The characteristics on which the largest differences between any two groups emerged in the solution shown on Table 1, [vi] and [x], are also the only characteristics where one of Garety et al's groups differed from both of the others by more than five points. In both investigations, furthermore, characteristics [iv] and [ix] produced two of the largest differences between any pair of groups (in the case of the present investigation 3.6 and 3.9 points respectively between the mean scores of groups two [SDs=3.3 and 2.1] and three [SDs=0.7 and 0.9]). Finally, as regards both Garety et al's and the present investigation the same characteristic, [i], produced the smallest differences across all three groups, this difference in the case of both investigations being less than 1 point.

Taking the mean scores of the two largest groups, 1 and 3 alone (these two groups together containing 50 of the patients in the present sample) differences of less than 1 point between the two groups were observed on six characteristics, and a difference of more than 4 points was observed on only one, characteristic [x]. The differences between these two groups on the other four characteristics were between 2.4 were 3.8 points.

Different types of OCD patient (cleaners, checkers and others) featured in each of the three groups shown in Table 2.F.1 (see section 2.1.6.1 for details as to the different types of patient included). Of the 26 patients whose major difficulties were to do with cleaning, 8 (31%) appeared in group one, 2 (8%) in group two and 16 (61%) in group three. The 23 patients whose major difficulties were to do with checking were similarly distributed - 6 (26%) in group one, 2 (9%) in group two and 15 (65%) in group three. As regards the remaining 6 patients, whose major difficulty concerned neither checking nor cleaning, 4 (67%) appeared in group three and 1 (16.5%) in each of the other groups.

2.2.3.3 The solution for characteristics [i]-[xiii]

Table 2.F.2 shows the "complete linkage" solution using characteristics [i]-[xiii] for three groups. This solution yielded clusters of 36, 16 and 3 members (respectively 65.5%, 29% and 5.5% of the sample) and was chosen from among the other "complete linkage" solutions because they either produced only one cluster which contained more than just two or three members (this was the case as regards the solution for two groups) or produced more than one cluster which contained just two or three members (this was the case as regards the solutions for four or five groups). All of the "single link" and "group average" solutions for characteristics [i]-[xiii], like those for characteristics [i]-[xi], produced only one cluster which contained more than a single member.

Although they produced three groups of very similar size, the solutions shown in

Tables 2.F.1 and 2.F.2 differed quite considerably as regards their distributions of individual patients. Of the 36 patients in the largest group in Table 2.F.2, group one, 24 (67%) were from the largest group in Table 2.F.1, group three, the other 12 (33%) being from group one. Of the 16 patients in the next largest group in Table 2.F.2, group two, only 3 (19%) were from the second largest group in Table 2.F.1, group one, 11 (69%) being from group three and two (12%) from group two in Table 2.F.1. All three of the patients in the smallest group in Table 2.F.2, group three, also appeared in the smallest group in Table 2.F.1.

The solution shown in Table 2.F.2 yielded clusters which contrasted with one another somewhat more than those in Table 2.F.1. The mean scores of groups one and two differed from one another by more than 4 points on characteristics [vi] (absurdity) (SD group one=2.1, SD group two=3.0) and [vii] (self evidentness) (SD group one=2.8, SD group two=2.1), and by more than five points on characteristic [xiii] (resistance 3) (SD group one=4.2, SD group two=3.1). Reasonably large differences of 3.6 and 3.1 points also emerged between the mean scores of these two groups on characteristics [xii] (resistance 2) (SD group one=4.2, SD group 2=4.1) and [i] (conviction) (SD group one=3.0, SD group two=1.4) respectively. No difference of greater than 1.3 points emerged between the means of these two groups on any of the other characteristics. These five characteristics on which groups one and two differed most made up the "perceived senselessness" (involving characteristics [i], [vi] and [vii]) and "resistance" (involving characteristics [xii] and [xiii]) components which emerged as separate from one another in some of the principal components analyses which were reported above (section 2.1.7). The patients in group one may thus be characterised as exhibiting a high level of perceived senselessness as measured by characteristics [vi] and [vii] (that is, high absurdity and low self evidentness), and a moderate level of perceived senselessness as measured by characteristic [i]. This group also exhibited a moderate level of resistance as measured by characteristics [xii] and [xiii]. group two, by contrast, exhibited a low level of perceived senselessness as measured by all three of the characteristics [i], [vi] and [vii] (that is, high conviction and self-evidentness, and low absurdity) and low levels of resistance as measured by characteristics [xii] and [xiii]. (Group three also differed from one or both of the other two groups by more than 4 points on several characteristics, but the small number of patients in this group means that these findings are of less interest).

The distribution of the different types of OCD patient across the three groups in Table 2.F.2 were very similar to their distribution across the groups in Table 2.F.1. Of the 26 patients whose major difficulties concerned cleaning, 16 (61%) appeared in the largest group, group one, 8 (31%) appeared in the next largest group, group two and 2 (8%) in the smallest

group, group three. Of the 23 patients whose major difficulty concerned checking, 15 (65%) appeared in group one, 7 (31%) appeared in group two and one (4%) appeared in group three. Of the other six patients, 5 (83.5%) appeared in group one and 1 (16.5%) in group two.

Table 2.F.1: "Complete linkage" cluster analysis for three groups using items [i]-[xi] on the characteristics of obsessive-compulsive experience rating scale - group means and standard deviations for each item

	Gr 1	Gr 1	Gr 2	Gr 2	Gr 3	Gr 3
	N=15	N=15	N=5	N=5	N=35	N=35
Item No.	Mean	SD	Mean	SD	Mean	SD
[i]	6.3	3.3	7.2	2.5	6.9	2.9
[ii]	5.5	2.8	6.0	3.4	9.1	0.9
[iii]	9.2	1.3	7.6	2.1	9.6	0.6
[iv]	9.2	1.3	6.0	3.3	9.6	0.7
[v]	8.6	1.8	6.7	1.6	8.9	2.0
[vi]	8.1	2.4	4.0	2.8	5.7	3.3
[vii]	4.9	3.5	7.3	0.9	4.0	3.2
[viii]	4.1	3.6	4.5	4.6	7.2	3.1
[ix]	9.1	1.0	5.5	2.1	9.4	0.9
[x]	9.6	0.5	6.8	1.9	4.3	1.0
[xi]	7.2	2.6	6.8	1.9	3.4	0.8

Table 2.F.2: Complete linkage cluster analysis for three groups using items [i]-[xiii] on the characteristics of obsessive-compulsive experience rating scale - group means and standard deviations for each item

	Gr 1	Gr 1	Gr 2	Gr 2	Gr 3	Gr 3
	N=36	N=36	N=16	N=16	N=3	N=3
Item No.	Mean	SD	Mean	SD	Mean	SD
[i]	5.7	3.0	8.8	1.4	6.7	3.3
[ii]	8	2.4	7.8	2.6	6.1	4.7
[iii]	9.4	0.9	9.2	1.3	8.3	2.0
[iv]	9.6	0.7	8.3	2.4	7.8	2.6
[v]	8.6	2.1	9.1	1.3	6.1	1.8
[vi]	7.8	2.1	2.9	3.0	5.1	3.3
[vii]	2.9	2.8	7.5	2.1	6.8	7.8
[viii]	6.1	3.7	6.5	3.5	3.6	4.8
[ix]	9.2	1.0	9.2	1.2	4.5	2.1
[x]	9.6	0.5	9.1	1.3	5.5	0.6
[xi]	8.5	2.0	9.0	1.2	5.6	1.5
[xii]	6.6	4.2	3.0	4.1	0.5	0.7
[xiii]	6.4	4.1	1.2	3.1	0.7	1.0

2.2.4 Discussion

The three groups identified by the cluster analysis shown in Table 2.F.1 do not differ from one another to a very great extent in terms of their scores on the various characteristics. When compared, for example, to the contrasting scores of Garety et al's (1988) three groups and those of the groups in Table 2.F.2, it is the similarity of the clusters in this solution to one another which is most striking. It is intriguing that the characteristics which produced the largest differences between the groups in this solution also tended to produce the largest differences between Garety et al's groups, and that the same characteristic in both cases produced the smallest difference between the groups. The significance of these similarities is, however, unclear. The features which distinguish groups one and two in Table 2.F.2 are among those included in the "standard diagnostic criteria" (see sections 1.2.2 and 1.2.6) for OCD and are central to much of the discussion of these criteria. As noted earlier (section 1.2.3), Schneider (1925) argued that for a thought or action to be symptomatic of OCD the patient must perceive it to be senseless; Lewis (1936) denied this, and instead suggested resistance to be crucial for the diagnosis. It would seem, therefore, contrary to the standard diagnostic criteria, and the positions of both Schneider and Lewis, that the patients in group two tended to report low levels of both these features (although see section 1.2.6 for a clarification of Lewis's position). Whatever the diagnostic interest of the present findings, however, there is no reason to suppose any of the differences between the groups identified in Tables 2.F.1 or 2.F.2 to be associated with such matters as, to take two examples from Garety et al's paper, differences in aetiology or treatment outcome. The present findings show the groups in both solutions to be unrelated to the distinction between checkers and cleaners.

2.2.5 Summary

Single link, complete linkage and group average cluster analyses using characteristics [i]-[xi] and characteristics [i]-[xiii] have been reported. None of the solutions examined for characteristics [i]-[xi] succeeded in seperating the patients into clearly distinguishable groups, although the "complete linkage" solution for three groups did so to the greatest extent. The "complete linkage" solution for three groups for characteristics [i]-[xiii] yielded clusters which were reasonably clearly distinguishable from one another in terms of the characteristics involving "perceived senselessness" and "resistance".

2.3 Part A conclusions

Both conceptual and empirical objections to the standard diagnostic criteria for OCD have been presented. Conceptual and empirical subdivisions of OCD symptoms and patients have also been offered, along with an examination of various characteristics of obsessive-

compulsive experience in OCD patients. It has been emphasised that it is as yet too early to say whether or not any of the suggested conceptual or empirical subdivisions are of any importance from the point of view of the actiology or treatment of OCD. In the discussion of the various theoretical approaches to OCD offered below in Part B of the thesis, therefore, no detailed reference to the subdivisions suggested in the present Part will be made, although the applicability of certain of the theoretical approaches which are to be discussed to (at most) only some of the kinds of OCD symptoms described in section 1.3 above will be noted. The distinction between checking and cleaning compulsions will be important to the empirical investigations reported in Part C of the thesis, and the distinction between groups one and two in the second cluster analysis reported above (see section 2.2) will also feature in one of those investigations (see chapter 6).

Part B: Theories of Obsessive-Compulsive Disorder

Chapter Three: A number of theoretical approaches to OCD

3.1 Introduction

Before presenting his own review of various theoretical approaches to OCD, de Silva (1988, p203) briefly discusses "what is known about factors influencing the aetiology of obsessive-compulsive disorders". Three factors which he suggests may be important are precipitating stresses, genetic influences and "social learning as mediated by parental modelling and parental control" (p214).

As regards the first of these factors, de Silva notes that in those cases where a specific time of onset can be established, such general stresses as "overwork, sexual and martial problems...[and] the illness [or] death of a close relative" (1988, p203) are sometimes present, suggesting that in these cases these difficulties may have helped to precipitate the disorder. However, Reed (1985, p70) suggests there is little consistency among reports as to which of these kinds of stress are most common at the onset of OCD, and in any case such difficulties as overwork and sexual or marital problems may themselves be at least in part produced by the onset of the disorder. de Silva (1988) also notes that in many cases no specific time of onset can be established.

Rachman and Hodgson (1980, p41) suggest that the evidence for a "specific genetic contribution to obsessional-compulsive disorder is inconclusive", the studies of relevance to this matter having failed to control for possible environmental influences. Thus, there is no adequate control for such influences as regards the findings of the two studies quoted by de Silva (1988) in support of a genetic contribution to OCD, these being Carey and Gottesman (1981) (who reported a higher concordance of OCD among monozygotic than dizygotic twins), and Murray et al (1981) (who reported a high concordance of "obsessionality" - traits and symptoms - among a large sample of normal twins). Environmental influences might evidently be able to explain both of these findings. Rachman and Hodgson point out that "there is plausible support for the argument that there is an important genetic contribution to general emotional oversensitivity or neuroticism", and suggest that "the possibility of a general genetic contribution [to OCD], through the vehicle of an increased predisposition to anxiety, or to neuroses generally, cannot be excluded" (1980, p41, original emphasis).

de Silva (1988, p203) also suggests that "social learning" may play a role in producing OCD, this being, according to de Silva (following Rachman and Hodgson [1980]), most likely to be by means of "general behaviour patterns such as overdependence and timidity, [which are] usually fostered by parental attitudes". But again, it is difficult to establish the importance of

such parental influences as these in the absence of studies which have controlled for a possible genetic contribution to the disorder.

The theoretical approaches to OCD which are to be reviewed in the present and following chapter are probably all equally consistent with these findings concerning the possible precipitants of, and genetic and "social learning" contributions to, the disorder as well as with the other details of what is known about its natural history (see section 1.1). While a variety of different approaches to OCD are examined in the present chapter, the discussion does not aim to be exhaustive and, in particular, it omits detailed consideration of such important elements as (1) the interesting relationship between OCD and depression (Stengel 1945, Lewis 1934), (2) neurological conceptions of the disorder based upon the observed similarities between patients with OCD and patients with various cerebral disorders (Turner et al 1985, Kettl and Marks 1986, Rapoport 1989), (3) the "serotonin hypothesis", and other explanations which have been advanced principally on the basis of the favourable response of some OCD patients to clomipramine (Insel 1984, Rapoport 1989) and (4) the "phenomenological" approach of Schneider (1925, trans. 1958) whose classic work on personality disorders includes a discussion of obsessionals, termed by him "insecure psychopaths". The present chapter also omits the consideration of the theoretical approaches which are to be empirically tested in the investigations reported below (see part C) (with the exception of some aspects of the account discussed in section 3.5); the accounts which are tested by these investigations in Part C are reviewed in chapter 4.

3.2 Behavioural/learning accounts of OCD

3.2.1 An outline of the model

The behavioural/learning account of OCD (e.g. Eysenck and Rachman 1965) is based on Mowrer's (1939,1965) "two factor" theory of learning. This approach provides very similar accounts of both OCD and phobic states. The preoccupations of OCD patients are held by this approach to concern feared stimuli which have acquired anxiety-producing properties by a process of classical conditioning. The compulsions which these feared stimuli provoke are, the behavioural/learning account suggests, escape or avoidance behaviour. According to this account, this behaviour reduces or prevents anxiety, and is thus reinforced by a process of instrumental conditioning. de Silva (1988) suggests that "the model is essentially one of learned anxiety reduction". In the case of OCD the avoidance behaviour may be, as in the case of phobias, passive, for example, not approaching objects which are thought to be contaminated or active, for example, checking for contamination (Gray 1982, p440). An example of an OCD escape behaviour is cleaning to remove what is thought to be some

contamination.

The model is not substantially altered by the recognition that it is not only anxiety but also other forms of mood disturbance, subsumed under the vaguer term "discomfort" (Rachman and Hodgson 1980) which is reported by some OCD patients. The maintenance of compulsions is then held to be by learned discomfort reduction (de Silva 1988).

Consistent with this account, Rachman and his co-workers (e.g. Hodgson and Rachman 1972, Rachman et al 1976, Rachman and Hodson 1980) have shown that compulsions are indeed often provoked by environmental cues and that when OCD patients are exposed to these cues they experience an increase in anxiety/discomfort. These workers have also shown that when these patients carry out their compulsive behaviour they often experience a considerable reduction in discomfort. This is cited by de Silva (1988) as support for the behavioural/learning model, as are the results of behaviour therapy with the disorder. Both of these points will be discussed below.

3.2.2 Preparedness and incubation

Eysenck (1979) has proposed a number of modifications of the behavioural/learning model in the light of several difficulties, including the following:

(a) the selectivity of the stimuli which elicit fear and discomfort in OCD and phobic states is not explained - far from being a largely random sample as the model evidently predicts, some stimuli are greatly overrepresented in OCD and phobic states e.g. dirt and spiders, while others, which seem very likely to be sometimes associated with aversive stimuli e.g. cars and guns, seem to be underrepresented.

(b) in a large number of cases there is no evidence for the stimuli which elicit fear/discomfort in OCD and phobic states having been paired, let alone repeatedly paired, with aversive stimuli at the onset of the disorder

(c) the stimuli which elicit fear and discomfort in OCD and phobic states do not undergo extinction despite their presentation being unaccompanied by aversive stimuli.

Eysenck (1979) attempts to meet these difficulties by supplementing the behavioural/learning model with the concepts of "preparedness" (Seligman 1971) and "incubation". According to the preparedness hypothesis some stimuli more readily acquire aversive associations due to the evolutionary advantage these associations are postulated to have bestowed. It would, for example, be suggested that a fear of spiders or snakes has conferred such an advantage by producing avoidance of life threatening members of these species. It would be similarly argued that phylogenetically novel stimuli which are connected with genuine dangers e.g. cars and guns do not appear in OCD and phobic states because they

have had no opportunity to become "prepared". It is, then, this which explains the selectivity of the fear/discomfort eliciting cues seen in OCD and phobic states and thus meets difficulty (a), according to Eysenck.

It is less clear what Eysenck's account has to say concerning difficulty (b). Perhaps prepared stimuli could be hypothesised to acquire numerous mildly aversive associations which summate to produce clinical levels of discomfort? In addition or alternatively, it might be hypothesised that clinical levels of discomfort are reached because mildly aversive associations are subject to incubation (see below). Either way, an attempt is made to explain how OCD (and phobic states) can arise without the stimuli which are involved in these disorders having been paired with stimuli which are highly aversive. But either way again, the account requires the stimuli which are involved in these disorders always to have been paired with stimuli which are to some extent aversive. It may be doubted, therefore, whether difficulty (b) has really been answered at all.

Eysenck's incubation hypothesis states that the fear/discomfort elicited by a conditioned stimulus may be augmented, rather than extinguished, by unaccompanied presentations of that stimulus and thus attempts to answer difficulty (c). A number of objections have been brought against the incubation hypothesis. Bersch (1980) argues that the evidence for incubation is very limited and that the concept in any case lacks, at its present stage of development, predictive power, an objection echoed by Gray (1979). Bersch thus concludes that the "...contribution [of the concept of incubation] to an understanding of 'paradoxical' failures of extinction [and] the etiology of neuroses [i.e. difficulties (c) and (b) here respectively] must remain in doubt" (p16).

3.2.3 The anxiety reduction hypothesis

As noted earlier, de Silva (1988) cites the finding that anxiety/discomfort is often reduced by compulsive behaviour as support for the behavioural/learning model. As de Silva also notes, however, in some cases compulsions fail to reduce, and may even increase, the patient's anxiety/discomfort. This is especially true of those patients in whose compulsions checking behaviour predominates (Rachman and Hodgson 1980).

Rachman and Hodgson (1980) suggest that anxiety/discomfort-increasing checking rituals are reinforced by their long-term consequences, for example, the avoidance of future disasters which the patient perceives them to confer. This is despite the increase in discomfort associated with these compulsions in the short-term, which these long-term benefits are argued by Rachman and Hodgson to counter-balance. But it is precisely the difficulty of ascertaining whether or not any such long-term benefits have been acquired which Rachman and Hodgson

also use (see below) to explain the doubting, repetitiveness and low levels of anxiety/discomfort-reduction which are associated with checking compulsions. These authors can surely not have it both ways.

The behavioural/learning view that anxiety/discomfort decreasing compulsions are maintained because of the patient's experience in carrying out these compulsions, that is, due to "learned anxiety reduction" may in any case be mistaken. This account appears to assimilate such behaviour too closely to that of animals in learning experiments (see de Silva 1988 for some examples of animal learning studies which he points out have been "cited as possibly relevant to OCD" [p207]). The consequences of carrying out e.g. some lever-pressing behaviour in a laboratory task is indeed often the major factor which may be expected to control a laboratory animal's performance of this behaviour. But much of the, for example, compulsive cleaning and checking of OCD patients may be contrasted with such behaviour in this respect. Thus, the belief that cleaning something will make it less contaminated or that checking something ensures that it is not in a dangerous state is shared by normals and OCD patients alike. In the case of OCD patients these beliefs are, therefore, unlikely to be supported only by the patients experience in carrying out, for example, cleaning or checking compulsions - it is reasonable to suppose, that is, that OCD patients will hold these beliefs at least in part for the same reasons that normals do. These beliefs provide a reason for cleaning something which is thought to be contaminated or checking something which is thought to be a possible source of danger. An OCD patient has, in consequence, reason for carrying out many cleaning or checking compulsions which is independent of the effects of these compulsions on his level of discomfort. It should be no surprise, therefore, contrary to the anxiety reduction hypothesis, that some anxiety/discomfort-increasing compulsions are maintained, and it may be further suggested that the anxiety/discomfort reduction which is brought about by the performance of many compulsions is not, contrary to the behavioural/learning view, a major factor in maintaining the performance of these compulsions. On the present view, that is, patients will usually clean things quite simply because they think that these things are contaminated, and that cleaning them will make them less contaminated. Similarly, they will usually check things because they think that these things may be in a state such that they are a source of danger, and that checking them will ensure whether or not this is the case. Discomfort reduction may well frequently result from the performance of such behaviour, but is not required to explain its performance or maintenance.

3.2.4 Behaviour therapy

de Silva (1988) also cites "the numerous treatment studies of obsessive-compulsive

patients using the exposure-response prevention paradigm" (p205) (e.g. Emmelkamp 1982, Foa et al 1985) as support for the behavioural/learning approach which helped to inspire the use of this paradigm with OCD. It has been argued, however, that many of the suggestions made by behaviour therapists as regards the remediation of OCD were anticipated by Janet (Pitman 1987, Coteaux 1990) on the basis of a quite different model of OCD, although some objections to this interpretation of Janet's are offered elsewhere (see section 3.4.7.3). The mechanisms by which exposure with response prevention may work with OCD similarly continue to be a matter for debate. Many or even most accounts of the disorder are able to provide some explanation for why this treatment should be effective with the disorder. Thus, while the effects of exposure with response prevention have been explained in terms of conditioning mechanisms (Eysenck 1979), accounts of these effects have also been offered in terms of innate fear mechanisms (Gray 1979, 1982), "perceptions of self efficacy" (Bandura 1978, Southworth and Kirsch 1988) and various other alternatives (see chapter 7). While it might be argued that any theory of OCD must be able to make sense of the effects of exposure therapy with response prevention with the disorder, therefore, being able to do so cannot in itself be strong support for that theory. It should also be noted that there are some authors who would dispute just how effective exposure with response prevention has been shown to be in treating OCD, mainly on the grounds that the existing outcome studies lack, according to these authors, appropriate control treatments (e.g. Montgomery 1990). Montgomery (1990) also argues that a recent study (Cottraux 1990) comparing fluvoxamine and exposure treatments suggests a relative lack of efficacy for exposure therapy.

3.2.5 The repetitiveness of some compulsive behaviour

Gray (1982, p441) suggests that the learning/behavioural model fails to explain why compulsive behaviours occur repetitively and suggests that a common explanation for this and the repetitiveness of obsessions should be sought. It is necessary to qualify this criticism because, as Rachman and Hodgson (1980) note, not all compulsive behaviours are performed repetitively. These authors, as noted above, distinguish those patients ("cleaners") among whom cleaning compulsions predominate from those ("checkers") among whom checking compulsions predominate and report that, as compared with cleaners, checkers tend to exhibit (a) more repetitiveness in their rituals (b) more doubts as to how effective their rituals have been (c) less relief from discomfort/anxiety from their rituals. According to Rachman and Hodgson and against Gray, therefore, it is not the repetitiveness of compulsive behaviours per se but rather the repetitiveness of checking compulsions which stands in need of explanation.

Having drawn these three distinctions between checkers and cleaners, Rachman and

Hodgson go on to offer an explanation of them in terms of the nature of the tasks involved in checking and cleaning difficulties. Thus, cleaners are trying, it is suggested, to remove a source of discomfort which is "already present and usually evident (visually or tactually)", while checkers, by contrast, are trying to reduce the probability of aversive future events, for example, the consequences of leaving plugs in sockets and lights on. Final confirmation, Rachman and Hodgson argue, as to the outcome of rituals is therefore more easily available to cleaners than to checkers, and this, they say, explains both (1) why checkers tend to show repetition, doubting and low levels of relief as regards their compulsions and (2) why their compulsions exhibit these features to a greater extent than do those of cleaners.

A number of objections may be brought against this account. Firstly, if cleaners are thought to be scared of illness and so on, which is regarded by them as the likely result of not cleaning well enough, future consequences are as involved here as much as they are in the case of checkers. Rachman and Hodgson would perhaps answer this point by arguing that cleaners have a fear of dirt rather that of the consequences of not removing dirt. Quite apart from the entirely post hoc character of this reply, however, it is in any case doubtful that it succeeds in rendering final confirmation more available to cleaners. Is it, for example, harder to make certain that all the plugs are out of their sockets in a room than it is to make certain that every last particle of dirt has been removed from one's hands? If anything, it should be easier to be certain that plugs are out, especially when one considers the notion of dirt and germs one cannot see, as stressed, for example, by adverts for soap powders.

But the checker knows, it might be objected, that even if all the plugs are out and switches are off etc., it is possible that the future events he is trying to avoid - his house burning down, for example - may still occur. This is true, but is not a good explanation of why the checker has doubts about, and repeats, his compulsions. If the future consequences he fears may still occur despite plugs being out etc., and it is this which preys on the checker's mind, it gives him no reason to doubt, and repeat, his checks. Such a fear would, if anything, lead him into checking other things, for example, fuse boxes, after he has checked plugs etc. Thus, while Rachman and Hodgson's account may be able to make sense of the low levels of relief reported by checkers, one must conclude that neither the tendency of checkers to have doubts about, and to repeat, their compulsions nor their tendency to do these things to a greater extent than do cleaners, has been satisfactorily explained.

de Silva (1988) refers to Teasdale's (1974) discussion for an explanation of the repetitiveness of compulsions. de Silva follows Teasdale in arguing "that compulsive behaviours may be considered as avoidance behaviours which are under poor stimulus control"

(p206). He suggests that these behaviours do not have "good feedback or safety signal properties" (p206), and it is their lacking these properties which explains, de Silva suggests, their repetitiveness. de Silva makes this suggestion as regards both cleaning and checking compulsions. Yet, if the earlier arguments are accepted, it is precisely those compulsions to which these points would seem to be most applicable, that is, those involving cleaning behaviours, which tend not to be repeatedly performed. Couching the earlier arguments in de Silva's terms, it is the poor detectability of possible sources of contamination which would appear to render the compulsive cleaner's task one with poor "feedback" or "safety signals", yet it is compulsive checkers who tend to repeat their behaviour.

The compulsive checker's task might also be argued, as de Silva suggests, to have poor "feedback" or "safety signals" - this is a repetition of the earlier claim that the possible future consequences the patient is trying to avoid may still occur despite, for example, all plugs being out and all switches off etc. But for the same reasons as those offered above this cannot explain, contrary to de Silva's suggestion, why checking compulsions tend to be repeated. The patient has, that is, in de Silva's terms perfectly good "feedback" or "safety signals" as regards the immediate outcome of those actions which he repeats. It is, objectively speaking, easy to establish whether or not plugs have been removed or switches turned off etc. and the patient's repetitive behaviour cannot be explained by the uncertain nature of - the poor "feedback" or "safety signals" he has with respect to - longer term outcomes.

3.2.6 Obsessions which are not provoked by environmental cues

As was noted above, the behavioural/learning approach provides very similar accounts of both OCD and phobic states. What, then, of those obsessions which, unlike phobic fear (and many obsessions), are not provoked by any environmental stimuli at all? Examples of obsessions which may not be so provoked are (a) images of horribly mutilated dead bodies (b) blasphemous thoughts (c) unacceptable insults concerning, for example, the appearance or conduct of the patient's partner or close relatives (d) number sequences and nonsense phrases which continually run through the patient's mind (see section 1.3 for a discussion of a closely related feature - the fear/discomfort in such obsessions having a "covert object" - as one of the ways in which OCD is by definition distinguished from phobic states).

All of these obsessions, then, may be such as to trouble the patient in any place or at any time. The occurrence of such obsessions, that is, may not be associated with the patient's having been or being in any particular kind of situation, for example, having had contact with what the patient thinks to be some contaminating material, being in the presence of any specific person etc. (such environmental provocations may, of course, exist for some

obsessions such as those in the above examples - all that is being claimed is that environmental provocations do not exist for all such cases).

Rachman (1978) provides an analysis of obsessions in general which may be applied to obsessions of this kind in particular. This analysis emphasises the similarities between obsessions, which are termed in this analysis "noxious [internal] stimuli to which patients fail to habituate" (p264), and phobic objects. Rachman suggests that obsessions, like phobic objects, give rise to anxiety and (overt or covert) escape and avoidance behaviour and may even be accompanied by physiological reactions similar to those elicited by phobic stimuli.

It seems that in the case of some obsessions not all of these hypothesised similarities to phobic objects hold good. Thus, as Rachman and Hodgson (1980,p274) themselves note, the analysis "provides little enlightenment about silly, insignificant obsessions such as number sequences, nonsensical phrases and the like" (see example [d] above). Rather than being anxiety provoking, it seems in such cases more likely to be the patient's frustration at having such trivial matter on his mind which is at the heart of his distress, contrary to Rachman's analysis. This difficulty for the behavioural/learning approach may also arise in the case of some obsessions which, in contrast to those at present in question, are provoked by environmental cues (although see Salkovskis 1985 for an attempt to explain such cases within an approach broadly similar to Rachman's).

But what of those cases where obsessions do exhibit the similarities stressed in Rachman's analysis, as seems likely to be the case in examples (a)-(c) above? Some objections are offered by Gray (1982). He argues that these "cognitive phenomena [obsessions] are not behaviour" but are, rather, "part of the systems that control behaviour". To suppose that these systems follow the same laws as behaviour is, Gray suggests, "the same kind of mistake as the belief that the neural display on the visual cortex is inspected by a second pair of eyes" (p441). Gray also argues that "if we treat internal events as behaviour, would we not expect noxious thoughts to be avoided passively, that is, not occur?"

Gray's first objection may probably be rejected. Any "explanation" of the functioning of the visual system in terms of "a second pair of eyes inside the head" would fail by leading to an infinite regress. It would seem that Rachman's analysis implies no infinite regress.

Gray's second objection states that Rachman's model treats internal events as behaviour, and yet it is, contrary to this objection, the supposed similarities between obsessions and external stimuli, not behaviour, which are stressed by Rachman's analysis. Nonetheless, Gray's objection introduces the important issue of how the occurrence of obsessions of the type in question is to be explained by Rachman's model. Thus, it seems that

any account which models obsessions on phobic stimuli cannot hope to illuminate the question of why obsessions of this kind occur at all, described by de Silva (1988) as a "relatively neglected aspect of obsessive-compulsive disorder" (p212). A patient's experiencing an obsession is supposed, on Rachman's account, to be analogous to a phobic patient encountering his feared environmental cues. Yet how the phobic patient encounters these feared cues - how, that is, his episodes of fear occur - is unproblematic. This is simply a matter of the patient being in a certain kind of place or in contact with a certain environmental cue. The occurrence of obsessions of the kind in question cannot, by the very nature of these obsessions, be explained in these terms.

This is not, of course, the only basis on which the value Rachman's model can be assessed. This might also be judged, for example, as regards the treatment suggestions to which the model gives rise, this being the respect in which Rachman's contribution has been most energetically followed up. The model has been presented as a rationale for using, for obsessions of the type in question, treatments which are adapted from the existing behavioural treatments for phobias (and for obsessions which are provoked by external cues). Efforts have been directed, therefore, by those working under Rachman's influence, to devising treatments for these obsessions which involve "exposure" to the patient's "noxious internal stimuli" while preventing the performance of any associated anxiety-reducing compulsions. Most recently the approach which has been adopted in the attempt to provide such a treatment is to play patients on 30 second loop-tapes the contents of their obsessional thoughts, recorded in the patient's own voice.

The support which the results from this approach have so far provided for Rachman's model is at best equivocal. Thus, while there are a few promising single case reports (Headland and McDonald 1987, Salkovskis and Westbrook 1989) in which this intervention has been used, Lovell et al (1991) report the first controlled investigation of this intervention, a small study with six patients in each of two groups. In this study, the intervention was compared with a control treatment in which patients were played recordings (once again in the patient's own voice) of "neutral nonaxiogenic material" - material, that is, which was unconnected with the contents of the patient's obsessions, such as passages of poetry. No significant differences between the effects of these two types of tape were found after eight weeks of treatment.

These authors also note, however, that four of the patients in the experimental group improved, as opposed to only one in the control group, these four experimental patients being those who found the taped contents of their obsessional thoughts anxiogenic. Despite the

absence of any significant difference between the two groups in Lovell et al's study, therefore, their results provide, as these authors note, some indication that recordings of the contents of obsessions may be a useful intervention for at least some patients. Further research is plainly required here, and it must meanwhile be concluded that definite confirmation of the efficacy of this treatment which Rachman's model helped to inspire has yet to be provided.

3.2.7 Emotional processing

Attempts to explain obsessions as conditioned emotional responses and/or as noxious internal stimuli to which patients have failed to habituate have been subjected to criticism above. de Silva (1988) goes elsewhere in the behavioural literature in order to find what he regards as "perhaps the best explanation for the persistence of obsessions, and of how normal intrusions become more chronic and achieve clinically significant levels of severity" (p212). This explanation is provided, de Silva suggests, by "the broad concept of emotional processing as proposed by Rachman".

Rachman (1980) introduces this concept in an attempt to help unify such apparently unrelated phenomena as obsessions, the "return of fear", abnormal grief reactions and nightmares. Rachman acknowledges many dissimilarities between these phenomena. But he believes there to be a need to establish "unifying concepts" given that some fear reducing procedures are able to produce comparable improvements in different forms of disorder, for example, exposure-based treatments for phobias, OCD and (possibly) abnormal grief reactions.

Another, quite separate, reason for introducing the concept of "emotional processing" is provided by Rachman. He suggests that while the undue persistence of fear (and therefore perhaps of obsessions too, in Rachman's view) is open to numerous explanations, "the unprovoked return of fear" following a diminution presents difficulties for "traditional theories". The return of fear implies that at least some of the original fear reduction must have been transient, and thus, Rachman suggests, "incompletely processed". This phenomena of the return of fear occupies much of Rachman's discussion in this paper.

Rachman proposes that obsessions, nightmares, the return of fear and numerous other phenomena be treated as indices of "unsatisfactory emotional processing" and that therapies for these various conditions be similarly formulated as attempts to facilitate the desired emotional processing.

Rachman is aware of the circularity of argument which may result from regarding phenomena such as the "return of fear" as both <u>resulting from</u> and being an <u>index of</u> unsatisfactory emotional processing. He believes this circularity to be avoided mainly by his proposal to use "test probes" - presenting patients, following the diminution of their fear, with

the stimuli which were formerly distressing for them. An independent measure of emotional processing is provided, Rachman suggests, by the degree of distress provoked by the presentations of these stimuli.

Rachman goes on to discuss factors that may give rise to difficulties in, and factors which may promote, emotional processing. Finally he raises a number of questions he believes to follow from the introduction of the concept of "emotional processing".

It seems clear that on at least the most obvious interpretations of Rachman's remarks they may be taken to represent a substantial departure from conditioning explanations of phenomena such as fear and obsessions. One is entitled to suppose that Rachman intends his analysis to be taken as such by the stress he places upon the "return of fear", a phenomena with which he evidently believes such explanations are unable to deal.

What, then, does this analysis put in the place of conditioning explanations? It would seem, unfortunately, not very much. Although plainly aware of the danger of circularity in his argument, Rachman seems to have failed to avoid it. In particular, it seems unlikely that "test probes" would be able to perform the function Rachman suggests. The "return of fear" is defined by him as "literally the reappearance of some degree of a fear that had undergone some diminution" (p54), and this surely implies that at the time this diminution was observed, the subject's response to Rachman's "test probes" would be more favourable, that is, would have involved less or no distress in response to the presentation of these probes, than it would following the "return of fear" - if this were not so, indeed, it seems unlikely that one would have been tempted to speak of a diminution of fear in the first place. The same seems to apply to the three other indices of "satisfactory emotional processing" suggested by Rachman (table 1, p55) - the decline of "subjective distress" and "disturbed behaviour" and the return of "routine behaviour" (for example, concentration) - reduction in these "indices" would be necessary before we should be prepared to speak of the patient's fear having diminished. One must therefore conclude that the "unsatisfactory emotional processing" which is reflected in the phenomena of the "return of fear" can only be judged to be present - we can only judge that "unsatisfactory emotional processing" has occurred - once fear has actually reappeared following a diminution. The concept of "emotional processing", in other words, fails in the form in which it is presented by Rachman, to cast any light on this phenomenon.

But if this is true of the concept as applied to this phenomenon, singled out by Rachman as pointing to the need for such a concept, it seems to be still more clearly true of its other applications in Rachman's paper. The numerous factors listed as promoting or impeding "emotional processing" (table 4, p57) are for the most part those which, as Rachman

himself notes, are "already familiar to behaviour therapists" (p57) as the favourable and unfavourable conditions in which to carry out behaviour therapy, for example, "long presentations", "repeated practise" (both favourable), "excessively brief presentations", "inadequate practice" (both unfavourable). These factors appear to be unified by Rachman only to the extent that the same term - "emotional processing" - is used in describing their effect. Similarly, most of the questions raised by Rachman at the end of his paper seem as if they could have been just as readily raised without reference to the concept of "emotional processing" at all. For example, the question (p58) "does [a subject's being engaged in a complex task when presented with a fearful stimuli] impede or facilitate the emotional processing [that will occur]..?" is simply the question "do such tasks effect the reduction in fear brought about by presenting fearful stimuli?" save for the introduction of the term "emotional processing".

Turning now to the case of obsessions in particular it must be acknowledged that this is not a topic which Rachman's paper discusses in any detail and for this reason it is not entirely clear from Rachman's paper what is supposed to be in need of "processing" in the case of obsessions - is it some experience separate from, but hypothesised to give rise to, obsessions or rather only the content of obsessions themselves? If the former, then this would only begin to be a theory of the origin and persistence of obsessions were some account of which experiences, and the mechanisms by which these experiences, produce obsessions to be provided. No such account is provided, and nor would it be merely by describing as "emotional processing" the relationship between obsessions on the one hand, and some experience hypothesised to give rise to obsessions on the other. If the latter, then it is once again not an explanation of obsessions which is being provided, but rather only a different way of talking about their varying degrees of severity - "this obsession is a sign of a good deal of unsatisfactorily processed emotion" being translatable, that is, as "this obsession is severe, will need a good deal of therapy" etc.

3.2.8 Summary

It has been argued that most of those aspects of the behavioural/learning account of OCD which have been considered here meet with substantial difficulties. This account is correct in suggesting that many obsessions and compulsions are provoked by environmental cues and that exposure to these cues causes many patients to experience discomfort. It has been argued, however, that Rachman and Hodgson's attempt to explain the repetitiveness of some compulsions from a behavioural point of view is implausible and that behavioural/learning theorists may have exaggerated the importance of discomfort reduction

to the maintenance of compulsive behaviour. Discomfort reduction is not necessary to explain the maintenance of such behaviour, and some brief remarks have been offered as to how a common explanation might be provided for the maintenance of both discomfort reducing and discomfort increasing compulsions.

The behavioural/learning approach encounters difficulties in explaining both the aetiology of OCD and the "paradoxical" failures of its "feared stimuli" to extinguish. Bersch argues that the concept of "incubation" fails to meet these difficulties and it has been argued here that the preparedness hypothesis also fails to do so. The question of how good an account of the selectivity of the stimuli seen in OCD can be provided by evolutionary arguments in general and the preparedness hypothesis in particular are offered below (see sections 3.3.4 and 3.3.5).

It has been noted that the behavioural/learning account provides only one explanation among many others as to why exposure with response prevention should be effective with OCD. Rachman's account of those obsessions which are not provoked by environmental cues has helped inspire a new approach to the treatment of such obsessions. The efficacy of this approach has as yet not been established, however, and it has been argued that Rachman's account of these kinds of obsession, in which the supposed similarities between them and phobic objects are stressed, fails to explain the occurrence of such obsessions. Doubts have also been raised as to the value of an alternative theoretical approach suggested by Rachman in which obsessions are seen as a sign of "unprocessed emotion".

Still other behavioural approaches to the treatment of OCD, and especially the use of "assertion training" with some instances of the disorder (Emmelkamp and van der Heyden 1980) are discussed below (see sections 3.4.7.1 and 3.4.7.3, and chapter 7).

3.3 Accounts of OCD based upon personality theories derived from the work of Pavlov

3.3.1 Introduction

Numerous authors writing under the influence of Pavlov's (trans. 1955) seminal contribution have stressed the importance of individual differences in personality (or "temperament") to the explanation of OCD, among other "dysthmic" disorders, for example, "neurotic" depression, phobias etc. Most notable among these authors is Eysenck, through whose work (e.g. 1952, 1979) this personality theory approach is linked to the behavioural/learning account of OCD and phobias (see section 3.2). A brief outline of this approach, along with some points of general relevance to it, will be followed by more detailed discussions of two recent contributions from this school, those of Gray (1982) and Claridge (1985) and in particular the accounts these authors offer of OCD.

3.3.2 Eysenck's account, and suggested modifications of it

As discussed above (see section 3.2.1), Eysenck sees OCD and phobias as resulting from the operation of conditioning mechanisms. Consistent with this, he holds that those people who are most inclined to develop conditioned responses, this being argued to be those who tend towards neurotic introversion, are those in whom these disorders are most likely to be observed.

Gray (1970) suggests, among other modifications of Eysenck's account, that it may only be in settings involving signals of "frustrative non-reward" or punishment that neurotic introverts are more inclined to develop conditioned responses.

Gray (1979, 1982) also denies, as does (to some extent) Claridge (1985), that it is the hypothesised greater inclination among neurotic introverts to develop conditioned responses which explains most cases of OCD and phobias. The preparedness hypothesis is thus rejected by these authors in favour of - particularly in Gray's account - an explanation of most cases of OCD and phobias in terms of higher levels of innate fear among neurotic introverts.

3.3.3 OCD and neurotic introversion

Both Eysenck and Gray argue, then, that it is neurotic introverts among whom OCD is most likely to be observed, and would thus agree with Claridge (1985, p72-3) when he states that patients with this disorder are "generally...introverted and highly neurotic on tests like the Eysenck questionnaires". This claim was not supported by the patients in the studies reported in Part C below. These studies included groups of 10 "cleaners" (OCD patients for whom cleaning compulsions were a major difficulty and who reported no checking compulsions of clinical severity), 10 "checkers" (OCD patients for whom checking compulsions were a major difficulty), 10 "psychiatric controls" (patients with various phobic difficulties), 10 "normals" (who had no psychiatric history) and 10 "ex-patients" (who had formerly been, but could no longer be, diagnosed as suffering from OCD). The scores of these groups on the Extraversion ("E") scale of the Eysenck Personality Questionnaire ("EPQ", Eysenck and Eysenck 1975) were not as would have been predicted by this school of personality theorists, and in particular it was found that the scores of the checkers (mean=12.7, sd=5.7), psychiatric controls (mean=9.2, sd=5.1) and normals (mean=12.3, sd=6.4) were much higher than those of the cleaners (mean=6.8, sd=4.2). It was found that the scores of the ex-patients (mean=8.3, sd=3.7) were somewhat higher than those of the cleaners, but not as high as those of the other three groups (five of the ex-patients had formerly been cleaners, three had been checkers). The difference between the cleaners and checkers was statistically significant ($p<0.05$), as was that between the cleaners and normals ($p<0.05$). The difference

between the psychiatric controls and cleaners fell short of significance. The checkers and psychiatric controls did not differ significantly from one another and neither of these groups was significantly different from the normals. There were no significant differences between the scores of the ex-patients and those of any of the other groups.

In this sample, then, neither checkers nor psychiatric controls confirm the prediction that low "E" scale scores will be recorded among patients suffering from dysthmic disorders, only the cleaners corresponding to this profile. The finding that among OCD patients it is checkers who do not confirm this prediction is especially surprising from the point of view of Gray's (1982) account (see section 3.3.6). The prediction that ex-OCD patients would score low on this scale was also not confirmed, although there was a non-significant tendency for scores in this group to be lower than those of the normals.

As regards their Neuroticism ("N") scale scores, the groups were more as would have been predicted by Eysenck's school of personality theorists. The cleaners (mean=19.3, sd=2.5), checkers (mean=16.9, sd=4.3) and psychiatric controls (mean=16.5, sd=5.2) all had high scores on this scale in contrast to the scores of the normals (mean=6.5, sd=5.2). The ex-patients (mean=14.8, sd=6.0) also scored much higher than the normals, although not quite as high as the other three groups. The differences between the cleaners, checkers, psychiatric controls and ex-patients on the one hand, and normals on the other, were all highly significant ($p<.01$). There were no other significant differences between the groups. (See Appendix A, tables A.1, A.5.1, A.5.2, A.6.1 and A.6.2 for further details of the group scores on the EPQ.)

3.3.4 Preparedness and innate fear

It was argued above that the "preparedness" and "incubation" hypotheses are unable to deal with two difficulties which confront behavioural/learning accounts - the lack of evidence for the stimuli which elicit fear/discomfort in OCD and phobias having been paired with aversive stimuli, and the failure of the stimuli which elicit fear/discomfort in OCD and phobias to undergo extinction. Neither of these two difficulties arise for an account which retains the evolutionary argument of the preparedness hypothesis but abandons the role which that hypothesis assigns to conditioning mechanisms (thus also obviating the need for Eysenck's [1979] "incubation" hypothesis). Such an account would, like Gray's (1979, 1982) and (to a lesser extent) Claridge's (1985), treat the fear/discomfort eliciting cues involved in OCD and phobias as innate.

In support of his "innate fear" position, Gray (1982 p429-34) argues that the findings from experiments conducted by Ohman and his co-workers, and presented by them in support of the preparedness hypothesis, are at least equally consistent with an innate fear account.

Gray further suggests (1979, 1982) that there is an observation which is consistent with the "innate fear" account but with which the preparedness hypothesis is unable to deal - this being that the onset of some phobias (and, one might add, much OCD) tends to occur at certain ages. Gray suggests that the preparedness hypothesis must suppose that conditioning experiences predominate at these ages to account for this observation, which is, he argues, no more plausible than the supposition that conditioning experiences tend to occur only in the presence of certain stimuli - and it was the implausibility of this latter suggestion which lead to the introduction of the preparedness hypothesis in the first place.

Against Gray's objection, a defender of the preparedness hypothesis might argue that the preparedness of stimuli has its onset at certain ages, which is no less plausible than the same suggestion as regards "innate fear", on which Gray's position is based. But this defence of the preparedness hypothesis would also need to suggest that potential conditioning experiences are common in order to explain why fear/discomfort will begin to be elicited by stimuli shortly after the onset of their preparedness. And against this, and in favour of Gray's objection, it has already been noted above that potential conditioning experiences are not common. It appears, therefore, that given this, Gray has indeed raised a further problem for the preparedness hypothesis.

Gray (1982, p434-5) also argues that, consistent with his account of human fear, there is an abundance of evidence for there being innate fears of a wide variety of stimuli in a diversity of species, "including fear of snakes in monkeys never before exposed to them". Against this Minneka (1986) has presented evidence which is, she suggests, more compatible with the fear of snakes among primates being prepared rather than innate.

3.3.5 Evolutionary accounts and OCD

As Gray (1982, p441) notes, and as was briefly commented upon above, a great regularity has been reported regarding the kinds of obsessions and compulsions which are observed both within and between "widely differing places". Thus, in both England (Rachman 1978) and New Delhi (Akhar et al 1975) the commonest obsessions concern dirt and contamination, the next most common in both places concern orderliness and aggression, followed by those which concern religion and sex. The most common compulsions reported in both England and India are also similar, involving cleaning, checking and tidying up. An attempt is made to explain some or all of this selectivity in exactly the same terms by the preparedness and innate fear accounts - in terms, that is, of the supposed evolutionary advantage conferred by fear/discomfort concerning the items in question. But how good an explanation can be provided in these terms? This question is fundamental to both the innate

fear and preparedness hypotheses.

Two preliminary points are worth noting. Firstly, all evolutionary arguments in this area tend to be of a highly speculative character, including those which are to be defended here - de Silva's et al's (1977) remark that "evolutionary arguments...are rather slippery and can be glibly made" is probably worth bearing in mind. Secondly, it seems on the face of things paradoxical to attempt an explanation of the very disabling degree of fear/discomfort observed in OCD in terms of some supposed evolutionary advantage which this level of fear/discomfort has bestowed. A far milder degree of fear/discomfort concerning the things which trouble OCD patients, such as that reported by many normals (Rachman and de Silva 1978), could perhaps be most plausibly argued to have survival value? If this is correct, an "evolutionary" explanation of OCD would have to be supplemented with some account of how the mechanisms which produce this optimum degree of fear/discomfort sometimes malfunction, resulting in clinical levels of distress. The force of the evolutionary argument appears to be weakened, but not entirely undermined, by this need for supplementary hypotheses.

So, how good an account of the selectivity of the stimuli and behaviour involved in OCD can be provided in evolutionary terms? de Silva (1988) suggests that the contents of most obsessions are indeed "highly prepared in the sense that they could be considered to be biologically relevant for humans in terms of their evolutionary significance" (for the purposes of the present discussion it is irrelevant whether this argument is couched in terms of the "preparedness" or "innateness" of fears). de Silva supports his assertion by citing the findings of de Silva et al's (1977) investigation but there are, as will be seen, difficulties regarding the manner in which that investigation classifies fears and behaviour as "highly prepared".

Before turning to that investigation, consider the general nature of evolutionary arguments in this area. To argue that a given fear or behaviour has been selected by evolution as innate or prepared it is not sufficient to point out that the fear in question is likely to have existed since antiquity. It must also be shown that this fear or behaviour could not have been "selected" on rational grounds. If it seems, that is, likely that "pretechnological man" (de Silva et al 1977) would have shared with modern man a fear of X, this is not yet enough to argue that this fear is innate or prepared. It must also be shown that "pretechnological man" could not have been sufficiently knowledgable to have appreciated a genuine danger which is posed by X. If such an appreciation can explain "pretechnological man's" fear of X then it is not necessary to postulate some further mechanism, such as the preparedness or innateness of X as a feared object to account for this. Thus, one would for this reason not be tempted to

suggest that, for example, lions and tigers are innate or prepared fears for human beings, however confident one might be that a fear of these animals would have been shared by "pretechnological man".

Consider, then, how one might argue that snakes or spiders are prepared or innate fear stimuli for human beings. The argument would be as follows: (1) while many kinds of snake and spider have posed a threat to the survival of human beings (and other mammals) over countless years, this threat is not "obvious", in the sense that it does not have to do with, for example, such physical characteristics as the size or strength of spiders and snakes (2) the acquisition by human beings (and other mammals) of innate or prepared fear regarding snakes and spiders would, therefore, be of value to survival in counteracting the absence of any "obvious" threat posed by the genuinely dangerously members of these species.

This argument does indeed appear to provide some reason for supposing the fear of snakes and spiders in human beings to have some evolutionary basis. It also makes it possible to explain, for example, why fears of snakes and spiders are found in children too young to appreciate the genuine threat which is posed by some members of these species and (of more relevance to the present discussion) why people are scared of spiders and snakes which they know to be harmless (in order to explain this it is necessary to assume that the mechanism by which innate or prepared fears are acquired is not sufficiently subtle to distinguish between threatening and non-threatening spiders and snakes).

Returning now to the investigation by de Silva et al (1977), this classifies as a "highly prepared behaviour" the "checking of fire hazards" and thus includes among such behaviour the very common OCD difficulty of checking light switches and plug sockets etc. for fear that a fire might otherwise be caused. The problem with this classification should be evident from the discussion above. It seems reasonable to suppose that a fear of fire hazards would indeed have been seen in "pretechnological man", but this seems reasonable precisely because it is very likely that "pretechnological man" would have been able to appreciate on rational grounds the threat posed by many such hazards. There is, therefore, no work here for prepared or innate fear mechanisms to do. Our reason for thinking that "pretechnological man" would probably have feared potential fire hazards is thus the same as our reason for thinking that this fear is not prepared or innate.

This is not to suggest, of course, that the OCD patient's checking of plugs and light switches is rational, and just a matter of the patient recognising the genuine threat posed by potential fire hazards. This is clearly not so - the endless repetition of such checking, the patient's inability to remember earlier checks, his need to carry out some exact number of

checks - none of these are merely a matter of the patient's recognition of genuine threat. But evolutionary arguments seem likely to provide at best weak explanations of such maladaptive behaviours as these. This claim is consistent with what has been argued earlier - it has already been suggested that evolutionary arguments are better at explaining why some stimuli are selected as aversive than they are at explaining very disabling degrees of fear of these stimuli, or indeed any other maladaptive behaviour with respect to them. Explanations of such fears and behaviour, including the repetitive compulsions and failures of memory etc. frequently observed in OCD patients with checking difficulties, must probably be sought in other terms.

As regards the question of why plug sockets, light switches and door locks are frequently selected as the items with which OCD checkers have most difficulty a quite straightforward answer seems most probable. These are the items which most people usually have to check in the course of an average day. Items which people who are not OCD patients rarely check, for example, fuse boxes, electrical wiring etc., are similarly more rarely problems for OCD patients (explanations couched in terms of the evolutionary significance of "potential fire hazards" are, of course, probably silent as to this contrast between light switches and plug sockets on the one hand and fuse boxes etc. on the other). The selection of plug sockets, light switches and door locks as problems for OCD checkers is, therefore, simply a matter of these being the things which most of us need to check - it is no surprise at all, therefore, that people with checking difficulties most frequently have problems with these things. It only needs to be added that this explanation of <u>what</u> OCD patients usually check unfortunately tells us nothing at all about <u>why</u> these patients have checking difficulties. Explanations of this must be sought elsewhere.

Evolutionary arguments may be somewhat more plausible regarding other OCD difficulties. Gray (1982, p443) argues that "the frequent choice of handwashing as the compulsive ritual" and the selection of dirt as "the commonest of obsessional preoccupations" reflect the fact that "natural selection may have favoured fear of dirt and its attendant grooming behaviour as much as fear of snakes or enclosed spaces" - "certainly", he suggests, "the danger to survival is no less great".

Why, then, is this argument more plausible than that concerning fire hazards? This is because the danger which dirt would be argued to pose to survival is, like that posed by snakes and spiders in the earlier examples, not obvious. It is a relatively recent discovery that dirt may carry germs and disease and "pretechnological man" would thus not have been sufficiently knowledgable, in contrast to the case of fire hazards, to appreciate the threat posed.

A problem can be raised here. As pointed out above, the reason for supposing that

"pretechnological man" would have feared potential fire hazards - his being able on rational grounds to appreciate the threat such hazards pose - is also a reason for doubting that such hazards are prepared or innate fears. The situation as regards the aversiveness of dirt is precisely the reverse. As argued above, an innate or prepared aversion to dirt would have been of survival value to "pretechnological man" because he would not have been able to discern on rational grounds the threat posed to his survival by dirt. But this also makes it far more difficult to be sure that "pretechnological man" would have had any such aversion.

It also seems to be difficult to produce very good evidence in favour of species other than our own finding dirt aversive. Thus, Gray (1982, p443) himself qualifies as "obviously speculative" his suggestion that there may be "...an evolutionary continuity between the human use of soap and water and...[the] grooming behaviour [of animals]".

Of greater interest would be evidence of OCD patients being preoccupied with dirtiness and cleaning in societies (past or present) in which it was (or is) not known that dirt carries germs (a milder aversion to dirt on the part of normals in such societies would similarly be of interest). This would still not, of course, amount to conclusive evidence that an aversion to dirt has been "favoured by evolution". Social and religious pressures within such societies, for example, might be argued to account for the "selection" of these preoccupations by their OCD patients. But the existence of these pressures in such societies may not weaken the evolutionary argument very much - these pressures would themselves be argued to reflect the innate or prepared status of the aversion to dirt in human beings.

Evolutionary arguments seem likely to provide only weak explanations of an array of other preoccupations and activities being common in OCD, such as the ordering of objects and "religious obsessions" (for example, blasphemous thoughts). Gray (1982, p443-4) tries to explain both of these, along with obsessions which are aggressive or sexual thoughts or impulses (see section 3.3.6), as reflecting various activities on the part of an hypothesised "Behavioural Inhibition System". But as is to be argued below, substantial difficulties confront the account of OCD in terms of the functioning of this system.

3.3.6 Gray on OCD

3.3.6.1 The Behavioural Inhibition System: an outline of Gray's account

Gray (1982) presents his account of OCD in the context of his theory of anxiety. The central psychological concept of this theory is the "Behavioural Inhibition System" (BIS). Much of Gray's investigation consists of a search for "the neural structures which might subserve the functions" (p424) ascribed to that system and at this level, according to Gray, the activity of the septo-hippocampal system partially corresponds to the functioning of the BIS.

Gray distinguishes between (1) the "comparator or monitoring capacity ("just checking")" (p425) of the BIS, in which it functions continuously but does not exercise direct control over behaviour, and (2) its functioning in those special conditions where it does exercise such control. The "chief function" (p442) of the BIS is performed in its "comparator" capacity, in which it monitors "ongoing behaviour, checking continuously that outcomes coincide with expectations". Incoming sensory information is scanned for "threatening or unexpected events" (p442). The "special conditions" in which direct control is exercised over behaviour are, on Gray's account, those where "threatening or unexpected" events are detected - those where, that is, "mismatch" is detected between outcomes and expectations. In these conditions, according to Gray, the system interrupts ongoing motor programmes, checks whether alternative programmes lead to more satisfactory outcomes and may in addition take control over exploratory and investigative behaviour (see p425). Gray also describes the BIS as increasing "attention to environmental stimuli and [increasing the] level of arousal" (p424) when it takes control of behaviour.

The account of OCD offered by Gray against this theoretical background is presented via a critique of behavioural/learning explanations. Gray points out that such explanations, as has also been argued elsewhere (section 3.2.6), fail to account for the occurrence of certain obsessions. He also suggests that a common explanation of the repetitiveness of obsessions and compulsions should be sought, and that behavioural/learning accounts fail to make sense of either. It is the functioning of the BIS, according to Gray, which provides such an explanation. As discussed above, the chief function of this system is to scan incoming sensory information for threatening or unexpected events. Gray explains that this scan involves certain stimuli being tagged as "important" and searched for with particular care. Were such a system to become hyperactive and thus "to tag too many stimuli as "important" [and search] for them too persistently", what else would it produce, Gray asks (p442), but the repetitive thinking and behaviour which is symptomatic of OCD? Thus, the OCD patient is observed, Gray points out, "to scan his environment to an excessive degree for potential threats...much of this scan being carried out overtly, in the form of checking rituals" (p442). (It is perhaps unclear from Gray's account whether it is the patient's checking behaviour, or just his urges to carry out such behaviour, which Gray hypothesises to be under direct BIS control.) This scan for potential threat can, Gray furthermore suggests, "extend to internal repositories of information concerning such threats" (p442), so that the patient who is anxious about cutting himself "checks his memory to verify where he disposed of a razor blade, or wonders whether he saw a splinter of glass on a table" (p443).

Gray does not restrict his account to those symptoms which would usually be thought of as involving the checking of the environment or "internal repositories of information". For example, handwashing, Gray suggests, "is at once an effective means of searching for dirt and a way to remove it" - the behaviour serves, that is, a checking as well as a cleaning function, according to Gray. The type of "internal scan" discussed above may also explain, Gray argues, the occurrence of impulses. The scanning of those systems which produce the behaviour which the patient is scared of carrying out may prime those systems to produce the behaviour in question, Gray suggests (p443), leading to the patient experiencing impulses to perform that behaviour.

3.3.6.2 OCD, anxiety and anxiolytic treatments

There are two points which Gray stresses in applying his theory to anxiety disorders in man. Firstly, that the strengths of his account are "most apparent when it is applied to the obsessive-compulsive neurosis" (p428) and secondly, that his theory has been derived from sources of data which are quite independent of that neurosis. These sources of data are "the results of purely behavioural experiments" with "the behavioural effects of the anti-anxiety drugs also [playing] an important part" (p424).

The second of Gray's points is especially important. According to Gray, the account of OCD which is provided by his theory might appear at first to be almost too good - it gives, Gray suggests, a "description [which] might seem too close to the phenomena to count as an explanation at all" (p442). It is the independence of the account's sources which ensure it an explanatory status and which make its close fit with the phenomena of OCD the "most apparent" strength of the theory. An especially noteworthy strength, according to Gray because, as compared with the phobias, OCD presents "a much higher hurdle" (p439) for theories of anxiety, a hurdle which other laboratory-based accounts (that is, behavioural/learning accounts) have failed to clear.

A prediction which seems to follow from these two points Gray stresses is that the "anti-anxiety" drugs should be effective in reducing the symptoms of OCD. This prediction seems, indeed, to be crucial to the theory - the effects of these drugs has played an important part in the construction of the theory, while the most important strength of this theory, as applied to anxiety disorders in man, has been presented as the account it provides of OCD. Yet the prediction is at best highly contentious. Thus, Fineberg (1990) has suggested that OCD differs from anxiety disorders in several fundamental ways including its failure to respond to "most conventional anxiolytic treatments" a claim with which Montgomery (1990) agrees.

Other contrasts emphasised by Fineberg (1990) are that OCD has, as compared with

anxiety disorders, a later age of onset and a different gender ratio - males and females being equally affected in the case of OCD, as against a female predominance in the case of anxiety disorders. Both Montgomery (1990) and Fineberg (1990) have suggested, on the basis of such contrasts between OCD and anxiety disorders, that the rationale for the current classification of OCD with these disorders is weak. It is of interest that doubts as to the status of OCD as an anxiety disorder have been independently raised on phenomenological grounds, for example, Reed (1985, 1990), Beech and Liddell (1974), Rachman and Hodgson (1980). This, then, raises another, albeit related, doubt regarding Gray's account, which seems to be committed, in contrast to behavioural/learning approaches (see section 3.2.1), to the major mood disturbance in OCD being anxiety.

3.3.6.3 BIS hyperactivity and checking behaviour

It is perhaps not very clear, contrary to Gray's suggestion (p442), that a "hyperactive" BIS would produce checking behaviour. Crucial to the present point is the distinction, drawn above, between on the one hand the monitoring functions of the BIS, in which it does not exercise direct control over behaviour and on the other hand the functioning of the BIS which takes place in response to "threatening or unexpected" events being detected, and in which direct control over behaviour is exercised. One must surely suppose that the behavioural and cognitive symptoms which will result from the BIS becoming hyperactive will be restricted to that behaviour and those thoughts which could be produced by the functions which the system performs when it takes control over behaviour. The monitoring of a hyperactive BIS would indeed involve, as Gray argues, too many stimuli being tagged as important and searched for too persistently. But this tagging and searching, it seems reasonable to suppose from Gray's account of the functioning of the BIS, would not be reflected by the patient's overtly scanning his environment in the form checking rituals, contrary to what Gray suggests (p442 - see above for quotes). This is because no reason has been given for thinking that the monitoring function of a hyperactive BIS would exercise direct control over behaviour. Yet it is precisely the similarity between the monitoring function of the BIS and such "scanning" behaviour on the part of OCD patients which is stressed by Gray (p442) and which gives his account of OCD in terms of the functioning of the BIS its apparent plausibility. To avoid the present objection, then, it seems that Gray must either (a) explain why in OCD the monitoring function of the BIS begins to control behaviour directly or (b) explain OCD instead in terms of those functions the BIS is ordinarily hypothesised to perform when it takes control over behaviour.

3.3.6.4 The BIS and Gray's dimensional approach

As discussed above, Gray suggests that in its monitoring capacity the BIS does not exercise control over ongoing motor programmes, such control only being exercised under conditions of "mismatch". It would seem, then, that the system would not be involved in ordinary checking behaviour such as that performed, for example, by a careful person locking his house at night. Such behaviour would rather be, in the terms of Gray's theory, an ongoing motor programme. It follows that Gray's account implies a qualitative difference between checking which is symptomatic of OCD, on the one hand, and normal checking behaviour, on the other - a qualitative difference in the sense that it implies different brain mechanisms subsume normal checking and checking which is symptomatic of OCD. The extent to which this implication counts in itself as an objection to Gray's theory is perhaps unclear - although it is worth noting de Silva's (1988) suggestion that much "obsessive-compulsive symptomatology does not seem to be qualitatively different from what is usually regarded as normal." But it seems clear that the implication of a qualitative difference probably conflicts with Gray's own position, which is (p426) that, as regards anxiety disorders (OCD being included here among such disorders) a dimensional approach is most appropriate. There are, that is, "continuous distributions of behavioural propensities" as regards such disorders, according to Gray, with "those individuals who need psychiatric attention simply [being] located near the extreme pole of one or other of these dimensions" (p426).

Other difficulties for Gray's account as applied to OCD have been raised elsewhere. de Silva (1988) argues that the view of "cleaning rituals as checking-cum-cleaning behaviours" conflicts with "clinical observations". de Silva also argues that, contrary to Gray's suggestion, the evidence does not really favour a unitary explanation of both obsessions and compulsions. This argument may be reinforced by the suggestion (section 1.3.4.5) that while there is no parallel to the repetitiveness of compulsions in the behaviour of phobic patients, the repetitiveness of some phobic thinking is indistinguishable from that of the obsessions experienced by some OCD patients. de Silva's objection may nonetheless be answered if it is only the urges to perform compulsive behaviours, rather than compulsive behaviours as such, which are being argued by Gray to be under direct BIS control (it was noted above that Gray's account appears to be ambiguous on this point). If it is only the urges to perform compulsive behaviours which are under direct BIS control, Gray's account can probably be reconciled with the contrasts between such behaviours and obsessions on which de Silva's objection is based.

3.3.7 Claridge on OCD

3.3.7.1 Anxiety in animals and man: Claridge's views on Gray

Claridge denies that a complete account of OCD or phobias can be provided solely in terms of the individual differences in temperament which are associated with anxiety - in terms of the differences, that is, which are central to the approach of Eysenck's school of personality theorists. Claridge therefore emphasises the necessity, as he sees it, also to include in any account of these disorders psychological processes which are unique to human beings, the physical instantiation of which will be, in contrast to the structures which are central to Gray's account, in Man's higher nervous system.

In suggesting this, Claridge takes himself to be rejecting Gray's claim that it is untenable to attempt any explanation of human anxiety in terms which are specific to Man. Thus, Gray argues on the basis of the "precise similarity of the brain mechanisms responsible for anxiety in both rats and men" (Claridge 1985, p72) that "any attempt to explain human anxiety in terms that are specific to man (by recourse, say to the vagaries of the Oedipus complex)" is "at once [ruled] out of court" (Gray 1982, quoted in Claridge 1985, p72).

Claridge seems to be correct to reject this claim - even if Gray is right that the brain mechanisms which subsume the experience of anxiety in human beings and animals are largely the same, this is quite compatible with the view - and this view is the position common sense would take with regard to the present question - that the things which human beings and animals become anxious about are not the same. Thus, the relative complexity or abstract nature of some matters - being in debt or having a terminal illness, for example - means that only human beings can understand them, and thus sometimes become anxious about them. It is reasonable, furthermore, to suppose that the capacity to understand such matters is unique to human beings because they are endowed with brain mechanisms which are peculiar to their species. But this supposition is entirely compatible with Gray's claim that the state of being anxious is itself mediated in human beings and animals by largely the same brain mechanisms. One must distinguish here between, on the one hand, <u>that which</u> a person (or animal) is anxious about and, on the other, the <u>anxiety</u> which is experienced by the person (or animal). (A possible source of confusion here is that one may describe as the "cause" of a person's anxiety both what the person is anxious about, and the operation of those brain mechanisms which mediate his anxiety.) This is not to deny that it may be possible, at a sufficiently general level, to describe in exactly the same terms what both human beings and animals become anxious about, and Gray (for example, 1982), indeed, attempts to do precisely this when he places many of the stimuli which provoke anxiety in both human beings and animals

under the common heading of "signals of punishment or frustrative non-reward". But this is entirely consistent with all of what has been said so far, and in particular does not deny that some signals of punishment and non-reward may be unique to Man. Thus, it seems that the "Oedipus complex", to take Gray's example, involves both signals of punishment - that is, from the father - and non-reward - that is, from the mother - despite the complex supposedly being such that if it were to exist, it would only be found in Man.

The fears observed in infrahuman species may nonetheless sometimes be used to help rule out those explanations of the fears reported by human beings which are couched exclusively in terms which are specific to Man. Thus, it is surely unparsimonious to attempt such an explanation of, for example, the fear of snakes which is reported by some people, given that this fear is also observed in some monkeys too. The most plausible explanation of such a fear in people will be one which is couched in at least partly the same terms as those which could be used to provide an explanation of this fear in monkeys too. But the force of this argument is derived, in contrast to that presented by Gray, from the fact that in this case it is not merely the brain mechanisms which mediate anxiety but rather the feared object which is the same for both humans and animals.

3.3.7.2 OCD - Claridge's account

Drawing on the work of Beech and Liddell (1974) concerning the nature of the mood disturbance in OCD as well as the work of psychodynamic writers, Claridge argues that OCD may result when "emotionally reactive" individuals - by which he means those who are highly neurotic and introverted - are "brought up in family environments that happen to place undue emphasis on cleanliness, sexual repression and control of anger" (Claridge, 1985, p74). This kind of upbringing may, according to Claridge, help to produce in such individuals the anxious guilt at sexual and aggressive feelings which, according to Claridge, produces much compulsive behaviour.

Claridge suggests that such anxious guilt may operate in conjunction with a characteristic cognitive style which is exhibited, he suggests, by people who are predisposed to OCD. Claridge draws at this point on the experimental work conducted by Reed (1969[i]) in which, according to Claridge, obsessional subjects were shown to have "more narrowly defined concepts" than controls. By inference from this, Claridge suggests that obsessionals may also have "a more narrowly focused mode of attending to stimuli" (Claridge, 1985, p75, original emphasis), this being evident, he suggests, in the preoccupation with minute detail often seen in OCD.

While Claridge (1985, p72) evidently regards as peculiar to human beings the

hypothesised individual differences in attentional style on which the cognitive aspects of his account are based, there seems to be no obvious reason for supposing that such individual differences in attentional style could not occur in other species (perhaps, indeed, the "anxious guilt" which Claridge also hypothesises to be at work in OCD could be more plausibly presented as peculiar to human beings?).

Of possible relevance to this point, Claridge himself notes that "there is an inextricable link between those mechanisms that have to do with the general arousal of the brain and the processes, in the higher nervous system, which control the breadth and narrowness of attention"; "the more emotionally aroused the organism is", Claridge explains, "the more focused its attention becomes" (1985, p75). Claridge therefore suggests that the extreme emotional reactivity which is, he argues, exhibited by OCD sufferers combines with the cognitive style which he also believes to be characteristic of these patients to produce the minute attention to detail which often features in their difficulties.

Claridge also briefly notes (1985, p76) that there is some evidence that subjects with obsessional personalities tend to show an "overdominance of the left [cerebral] hemisphere" which, he suggests, "through its strong association with the control of language, could account for...[the] intellectualised, repressive stance on the world" of such personalities.

Claridge's suggestions may be challenged on a number of points. Thus, his discussion fails, like Reed's (1985) account, to make sense of the numerous types of OCD symptom which fail to exhibit the characteristics stressed in Reed's analysis (see section 4.6.1). The experimental evidence available is in any case at best equivocal as regards Reed's suggestion that OCD patients are characterised by the cognitive style described in his analysis, and there is furthermore arguably no evidence at all, contrary to both Claridge and Reed, that this cognitive style plays any role in producing the suffering of OCD patients (see Chapter 4, 5 and 6). Regarding the experimental work Claridge cites in connection with both the hypothesis of left hemispheric overdominance in obsessionals and Reed's account, Claridge assumes too close a link between the obsessional personality and OCD (Pollack 1979, 1987).

Claridge's discussion nonetheless possesses at least two important strengths. Firstly, there is some evidence (Emmelkamp and van der Heyden 1980) which suggests that, consistent with Claridge's account, the manner in which some OCD patient's deal with their aggressive feelings may be important in explaining their symptoms (see Chapter 7). Secondly, as is argued elsewhere (see section 4.3), accounts of OCD such as Reed's which are based on the hypothesised cognitive style of patients with the disorder encounter difficulties in explaining the motivation of OCD patients, suggesting that such accounts can at best provide

only partial explanations of the disorder. Claridge's suggestions may be regarded as an interesting, if not entirely successful, attempt to provide such an account with the supplement it requires.

3.3.8 Summary

Both Gray's account and Claridge's suggest that OCD patients should tend to be introverted, but evidence has been presented which contradicts this claim. Among OCD patients introversion appears from the evidence quoted earlier to be exhibited more by those patients whose major difficulty is cleaning than it is by those whose major difficulty is checking. If this finding turns out to be replicable, it will require both Gray's account and Claridge's to be modified. This, of course, leaves open the possibility that the neuroticism of OCD patients may play a role in producing their symptoms as writers such as Eysenck, Claridge and Gray suggest and indeed it may even be, once again consistent with the views of Eysenck, Claridge and Gray, that in those OCD patients in whom both introversion and neuroticism are observed, these two features combine to play a role in producing the symptoms observed. But even if all of this is accepted, it is clear that such temperamental characteristics as neuroticism and introversion cannot deliver a full account of the symptoms reported by OCD patients. Thus, as has already been noted, these temperamental characteristics are supposed to be exhibited not only by patients suffering from OCD but also by patients with other "dysthmic" disorders (see, for example, Claridge, 1985, ch. 5) and so these characteristics could not begin to explain why, for example, one patient develops neurotic depression or a phobia while another develops OCD. Similarly, there appears to be a wide gulf between, on the one hand, the mere "emotional reactivity" which is supposed to characterise neurotic introverts and, on the other, the bizarre preoccupations, repetitive behaviour and failures of memory etc. which are reported by many OCD patients.

It is this gulf which Gray's account attempts to bridge while Claridge tries both to do this and to explain - in terms of individual differences in attentional style - why some patients develop OCD rather than phobias or depression. Both of these accounts, it has been argued, meet with difficulties. Gray's appears for a number of reasons not to provide a plausible explanation of the various OCD symptoms he discusses, including those which involve checking difficulties, the phenomena for which this account appeared at first to provide an especially elegant explanation. It has been noted that Claridge's account possesses a number of important strengths but relies too heavily on Reed's formulation of obsessional difficulties and thus encounters a number of the problems faced by that formulation.

What may be the most powerful argument considered in this discussion of personality

theories derived from the work of Pavlov owes very little directly to those personality theories - this argument being that it may be possible to explain the contents of some OCD symptoms, particularly those involving, as Gray suggests, fears of dirt/contamination and/or cleaning behaviour, in terms of the evolutionary advantage which such fears and behaviour may be hypothesised to have bestowed. It was also noted, however, that the contents of various other OCD symptoms could not be explained in these terms. In those cases where evolutionary arguments do possess some force, it was suggested that, once again consistent with Gray's and some of Claridge's remarks, the most plausible mechanism through which such evolutionary pressures could exercise this influence would be innately acquired rather than prepared fears.

3.4 Janet on OCD

3.4.1 Introduction

Janet's (1903) classic contribution to the literature on OCD has still not been translated into English. Fortunately, Pitman has recently provided a highly readable synopsis (1984) of Janet's work as well as an interesting review/commentary (1987[i]) which will be used as the basis of the discussion here. In his review/commentary, Pitman presents a summary of Janet's ideas in three segments, each of which will be considered in turn here; the points Pitman himself offers will be considered in the course of commenting on Janet's work. Havens (1966) also provides an interesting account of Janet's contribution, some points from which will be briefly discussed in what follows, as will some of Reed's (1985) comments on Janet.

3.4.2 Janet on the clinical stages of Psychasthenic Illness.

Janet included OCD, among other conditions, in the category he termed "psychasthenic illness". He divided this category into three stages, these being, from the least to the most severe, "the psychasthenic state", "forced agitations" and "obsessions and compulsions". According to Janet, patients develop the less severe stages of the illness before the more severe and similarly lose the more severe stages of the illness before they lose the less severe.

The first stage of the illness, the psychasthenic state, involves the patient in feeling that actions have been unsatisfactorily or incompletely performed; incompleteness in "perceptions" is also present, consisting of such experiences as derealisation and depersonalisation. Indecision, amnesia, poor control of thoughts and "emotional insufficiencies", that is, an inability to experience emotions fully, are reported by patients. A number of "physiologic insufficiencies" are also present, for example, insomnia, sexual impotence and backache.

The second stage of the illness, forced agitations, includes "mental phenomena", for example, rumination and repetition, "motor phenomena", for example, tics and agitations and

"emotional phenomena", for example, phobias and anxiety. The third and most advanced stage of the illness, "obsessions and compulsions" usually involve forbidden thoughts or acts of a sacrilegious, violent or sexual nature.

3.4.3 Commentary on the clinical stages of Psychasthenic Illness

3.4.3.1 The psychasthenic state and compulsive personality disorder

Pitman notes that Janet has been presented as holding that all OCD patients have premorbid obsessional personalities. Pitman suggests that this is not quite accurate. He accepts that most of the elements in DSM III's definition of compulsive personality disorder are included in Janet's account of the psychasthenic state and in the majority of cases this state would be held by Janet to have endured long enough to be classifiable as what today would be termed a personality disorder. But Janet also observed, Pitman points out, that the psychasthenic state may develop acutely and thus in these cases can probably not be classified as a personality disorder. It is also worth noting that Janet includes much in the psychasthenic state which appears to have nothing to do with the compulsive personality or personality disorder at all, for example, experiences of derealisation and depersonalisation.

3.4.3.2 Psychasthenia, OCD and other neurotic disorders

Pitman notes that among the forced agitations Janet included symptoms which would today be given quite separate diagnoses, including agoraphobia, panic disorder, social phobia and generalised anxiety. He suggests that Janet's inclusion of all of these disorders under the common rubric of "forced agitations" is supported by modern evidence of the co-morbidity of these disorders with one another and with OCD (Pitman, 1987[i], p228). This stress on the similarities between OCD and the disorders with which it tends to be co-morbid is an interesting similarity between Janet's theorising and that of the Pavlovian "personality theorists" (see section 3.3).

Pitman misrepresents Janet's position in suggesting that symptoms which would today be diagnosed as OCD appear only in the third and most severe stage of psychasthenic illness and it is, therefore, surely unfortunate that only the symptoms seen in this stage are termed "obsessions and compulsions". Thus, Pitman gives as examples of "forced agitations" in his review/commentary symptoms which involve checking, repetition, preoccupations with order and symmetry, a case involving "touching the pants to ward off the idea of a having been brushed by a rabid dog" (Pitman, 1987[i], p227) and a case of "a knife phobia [sic] [which arises] out of an obsession with the act of homicide". It seems likely that these symptoms would all be diagnosable as OCD and many similar examples are provided in Pitman's synopsis of Janet's work. Indeed, it is surely possible that some of the examples which Pitman

provides of the symptoms seen in the psychasthenic state, for example, feelings that actions have not been performed well or completely could in some cases be symptomatic of OCD. This is particularly so given the point noted above which Pitman himself makes, that such features are in some cases not classifiable as compulsive personality traits because of their acute onset.

Pitman seems, therefore, to be similarly in error when he reviews as crucial to Janet's position the modern evidence concerning the question of whether or not patients only report symptoms of OCD after they have begun to exhibit such disorders as anxiety-depression, phobias and generalised anxiety disorder (Pitman concludes that the modern evidence is equivocal on this point although "more consistent [i.e. with what Pitman takes Janet's position to be] than contradictory" (Pitman, 1987[i], p228)). Janet would appear to be at most only committed to the claim that some kinds of OCD - those involving "thoughts of a sacrilegious, violent or sexual nature" - should occur only after the appearance of (a) the other disorders which Pitman discusses as parts of the second stage of psychasthenic illness and (b) the kinds of OCD which, contrary to Pitman's interpretation, Janet evidently included among the second and even the first stages of psychasthenic illness. It also seems to be the case, from Pitman's account of Janet's work, that not only instances of what today would be diagnosed as OCD are included in the third stage of psychasthenic illness (although see the discussion of Haven's work in section 3.4.7.1). Thus, one example provided in Pitman's review of this stage of the illness involves "the belief that one is fat when one is actually thin" leading to "bizarre dieting with weight loss".

3.4.3.3 Psychasthenia, OCD and anxiety

Pitman stresses that Janet rejected "anxiety centred" explanations of OCD, preferring an account in terms of the "lowering of psychological tension" (see below). Pitman suggests that within Gray's (1982) broader neuropsychological theory of anxiety (see section 3.3.6) it may be possible to explain a number of the observations Janet thought "anxiety centred" accounts were unable to handle.

3.4.4 Janet on the Hierarchy of psychological phenomena, and psychological tension

Noting that not all psychological operations are equally impaired among psychasthenics, Janet constructed a hierarchy of such operations. Those operations which are at the top of this hierarchy are impaired in psychasthenic illness, those which are lower down are not. The highest level of the hierarchy is termed the "reality function" or "the function of the real" (Havens 1966) by Janet. It seems to be difficult to state in just a few words what Janet meant by this. What Pitman says is: "The highest level of the hierarchy is the reality

function. This includes real actions that require high effort such as social adjustment and adaption to novel circumstances. Alongside real actions are real perceptions, including memories pertinent to the real situation at hand, but not irrelevant ones. Also included are emotions adapted to present reality, especially happy emotions. Psychasthenics encounter numerous difficulties in dealing with real situations. They are rendered impotent by their shyness and have difficulty expressing even gratitude or tenderness...Novel social challenges are the most difficult for psychasthenics" (Pitman 1987[i], p229).

Below the reality function in the hierarchy are "disinterested mental activities". Janet gives as an example of these the giving of advice to others with which, he suggests, psychasthenics often have little trouble despite being unable to make their own decisions (the latter activity being included at the "reality function" level by Janet). Lower still in the hierarchy are thought processes having little to do with "present reality", for example, imagination and abstract reasoning. Lower still are "non-specific emotional-viscerosomatic discharges typified by anxiety" (Pitman, 1987[i], p229). Nonspecific motor agitation and tics occupy the lowest level.

Janet suggested that "two essential features characterise the highest level of the hierarchy: unification of novel mental synthesis and the richness of conscious elements that take part in it" (Pitman, 1987[i], p229). Examples of these "two essential features" are provided by Pitman insofar as we are told that it is a diminution in the capacity for "novel mental synthesis" which explains the psychasthenic's difficulties with volition and attention, and a "loss of the richness of conscious elements" which produces the experiences of depersonalisation and derealisation. This combination of these "two essential features" constitutes the characteristic Janet termed "psychological tension", which he believed corresponded to "some physiologic tension" in the central nervous system. The amount of psychological tension possessed by an individual determines the highest point in Janet's hierarchy at which that individual can function. Janet suggested that psychological tension is lowered in the psychasthenic, and it is this which makes the completion of higher level operations impossible for him.

The inability to complete high level operations leads to the appearance of phenomena such as agitations, tics and anxiety. This is, on Pitman's account of Janet, a result of the "mental energy", which would otherwise be used in the higher operations, being diverted into psychological operations which are lower down in Janet's hierarchy. The forced agitations in psychasthenia are thus said to arise when the patient wishes to perform some high level activity such as initiating an action or making a decision.

3.4.5 Commentary on the Hierarchy of psychological phenomena, and psychological tension

3.4.5.1 Psychological tension

Pitman makes an important point when he says that Janet's theory of "psychological tension" appears to rest upon a circular argument. We seem to be told by Janet that the reality function is impaired in psychasthenia because these patients have lowered psychological tension, while the reason Janet gives us for supposing the reality function to involve high psychological tension is that it is impaired in psychasthenia. Pitman's charge of circularity against Janet might be answered if the theory of psychological tension were applicable to conditions other than psychasthenia - indeed, as Pitman points out, one is entitled to expect the theory to be applicable to other conditions given that it is supposed to reflect "a basic organisation of the central nervous system" (Pitman, 1987[i], p230). But on the contrary, as Pitman again correctly observes (1987[i], p230), the theory appears precisely to lack such a wider applicability. Pitman points out that mentally handicapped people, for example, often have less difficulty with interpersonal relationships than with abstract reasoning, although it is interpersonal relationships which stand higher in Janet's hierarchy. Pitman also suggests that Janet's speculations regarding "some physiologic tension" in the central nervous system corresponding to the level of "psychological tension" observed would not be taken seriously by modern neuroscientists. Reed (1985), however, offers a quite different account of Janet's notion of "psychological tension" (see section 3.4.5.4).

3.4.5.2 The reality function

There may be difficulties for Janet's suggestion that all and only the functions which are included at the highest level of his hierarchy can be characterised as having to do with the negotiation of "reality". The claim that some level one functions may be contrasted with some lower level functions in this way seems reasonable (despite Janet's unusual choice of terminology). For example, it probably is plausible to claim, as Janet did, that adjusting to novel social circumstances (a level one activity) can be contrasted in these terms with abstract reasoning and introspection (level three activities). But in what way are, for example, the sleeping difficulties of psychasthenics supposed to reflect a failure of the "reality function"? Havens (1966, p394) informs us that Janet regarded such difficulties as reflecting a "failure of the act of sleeping". Yet even if one were to make the (substantial) concession that falling asleep is an act, this takes us no nearer an understanding of how the performance of this act involves the negotiation of "reality". Nor, arguably, is it clear how the giving of advice to others (a level two activity) involves less interaction with "reality" than social adjustment (a

level one activity).

It is surely also implausible to suppose that all OCD patients exhibit all of the features included in Janet's psychasthenic profile. Thus, the difficulties of psychasthenics "in dealing with real situations" leads, Janet suggests, to their being "rendered impotent by their shyness" - according to Janet, these patients "find on the stairway the word that needed to be said in the parlour" (Pitman, 1987[i], p229). Janet also suggested that "if there is anything that [psychasthenics] find more painful than a decision, it is a fight" (Pitman, 1987[i], p230). Memorable though these phrases are, one is, contrary to them, rather struck by how much these patients differ from one another with respect to such characteristics as their degree of shyness and timidity. Similarly, the scores of OCD patients were found (section 3.3.3) to vary considerably on some of the dimensions of personality studied by theorists working under Pavlov's influence (for example, Eysenck [1979], Claridge [1985] and Gray [1982]. Lewis (1936, p328) also remarked that not all obsessional personalities are timid. While, according to Lewis, some certainly are, being "vacillating, uncertain of [themselves and] submissive" others, Lewis suggested, are by contrast "obstinate...[and] irritable". Indeed, Janet himself remarked that psychasthenics may tend towards "authoritarianism" as well as "subordination" (Pitman, 1987[i], p227).

Janet regarded the activities which he placed at level two in his hierarchy - such as the example above of giving advice to others - as intact in psychasthenia, and it is therefore evident that he thought of only the highest level of his hierarchy as impaired in the illness. It is this selective impairment which Janet used to justify his placing the activities which he supposed to be exercises of the "function of the real" at the top of his hierarchy, and it is therefore unclear, at least from Pitman's account of his work, on what basis Janet organised into levels the other "psychological operations" he discusses.

3.4.5.3 Psychasthenic illness and Janet's Hierarchy

Janet makes no suggestion of the more advanced stages of psychasthenic illness involving activity which is lower in his hierarchy than those involved in the earlier stages. Thus, Janet appears to have held that tics and other motor agitations (both included in stage two) occupy the very lowest level of his hierarchy; the "obsessions and compulsions" of stage three, understood as including thoughts of an aversive nature for the patient, can evidently not be included at this level and must therefore appear higher, perhaps most plausibly at level three. The later stage of the illness thus seems to be, if anything, characterised by activity which is higher in Janet's hierarchy than much of that which is seen in the earlier stages.

3.4.5.4 Displacement activity

Janet's suggestion that forced agitations result from the diversion of energy intended for other acts was, Pitman argues, a remarkable anticipation of "the ethologic concept of displacement activities". Such activities "are currently considered a possible animal model of human compulsive behaviours", Pitman (1987[i], p230) notes, and adds that these activities "typically occur in conflict situations". An example of "displacement activity" Pitman gives involves the stickleback fish, which has been observed to engage in nest building activity while in the midst of a boarder dispute with a rival, this nesting behaviour being hypothesised to result from there being, in the stickleback which exhibits this behaviour, a conflict between the inclination, on the one hand, to fight and, on the other, the inclination to flight. Pitman stresses that this "displacement behaviour" is out of context, in that it is such as not to be attributable to either competing inclination, in contrast (in the case of the present example) to fight or flight behaviour.

It is at this point that Pitman finds an element missing in Janet's psychological formulation - Janet fails, in Pitman's view, to recognise "the role played by intrapsychic conflict" (1987[i], p228) in OCD. Pitman argues elsewhere (Pitman 1987[ii]) that most cases of OCD can be seen as displacement arising from "intrapsychic conflict" over the expression of, for example, aggression and sexuality. Pitman thus suggests that OCD usually occurs because the patient is motivated to act in ways he finds unacceptable. This produces conflict which in turn, Pitman suggests, gives rise to OCD and in particular compulsive behaviours, such behaviours being formulated by Pitman as displacement activities.

Pitman points out that Janet, by contrast, sees conflict and dissatisfaction as secondary - the result of volitional difficulties not the cause of them. The picture of Janet's account which thus emerges here is one in which half of the "conflict/displacement" model to which Pitman subscribes is provided. Janet is presented as having acknowledged the importance of displacement mechanisms while having failed to provide any successful account of why they operate and in particular of not having recognised the role played by "intrapsychic conflict".

A close reading of Pitman's own synopsis (1984) of Janet's work suggests that this picture overstates the similarities between Janet's work and Pitman's "conflict/displacement" model. It seems clear that Janet sees some of the symptoms of psychasthenic illness as arising by means of displacement mechanisms as described by Pitman. Thus, Janet talks of the necessity "to introduce...the concept of the diversion of mental energy, occurring when the primary avenue is blocked" and stresses that "various inferior phenomena...may substitute for one another in forced agitations, as if excitement had to be discharged somehow" (Pitman,

1984, p307).

But not all symptoms of the illness, nor even all forced agitations, are explained in these terms by Janet. Thus, he says that "the mania of repetition arises from the feeling of discontent with the way an action was previously performed, as does the mania of going back (checking)" (Pitman, 1984, p295). It is important to note again here that it is only actions the performance of which involves the "reality function" which are affected by such feelings of discontent etc. in psychasthenic illness. Thus, it would seem that the repetition and checking involved in symptoms such as those referred to in the above quote are of some action the successful performance of which features at the highest level of Janet's hierarchy, and which the patient is unable to perform successfully in the sense that he cannot experience any satisfaction as to the manner in which it has been carried out. This is, of course, entirely consistent with Janet's claim that the successful performance of these actions - meaning their being performed in the absence of such feelings of discontent etc. - is unavailable to psychasthenic patients. But this account seems to be clearly distinguishable from one based upon displacement mechanisms, in which checking or repetition results from energy diverted from some entirely different act the patient has been unable to perform due to some conflict. Similarly, there is no question here of the patient wishing to perform some action which he also regards as unacceptable in cases of the type to which Janet is referring in the above quotation about "manias" of checking and repetition. And nor is there, therefore, any suggestion of the conflict to which the wish to perform such an act would give rise being what makes acts of the type in question impossible for the patient to perform, the resulting repetition and checking being, as would be required by Pitman's conflict/displacement model, of some entirely different and "out of context" behaviour. The checking and repetition in the type of cases Janet is discussing in the quote above are of the very actions the successful performance of which is unavailable to the patient. This action is thus not of a forbidden nature for him - it is not, that is, an action he would not try to carry out (his repetition and checking are his attempts to carry it out) - and the patient's checking and repetition are thus similarly not of an action which is "out of context" in Pitman's sense.

Reed's (1985) account of Janet's work may be more consistent these observations. Reed (p78), in contrast to Pitman, sees Janet's notion of "psychological tension" as being a major advance in the understanding of OCD. Reed believes the notion to have been widely misunderstood, Janet having used it, Reed argues (p76), to mean "input integration" or "schematization". Reed thus believes there to be a close connection between Janet's account and his own (see section 4.1), in which the central problem in obsessionality is argued to be

a "failure in spontaneous categorizing and integration" (1985, p220). Reed regards (some) compulsive checking and repetition, for example, as resulting from the OCD patient's failure to categorize tasks as having been done "properly" or "sufficiently", this in turn resulting from the patient's inability to treat as irrelevant to this categorization trivial details as to, for example, the exact manner in which tasks have been performed - the patient is unable, in Reed's terms, spontaneously to categorize (or "integrate") the task and this is what, according to Reed, Janet was also claiming when he hypothesised that "psychological tension" is lowered in psychasthenia.

Whether or not this is plausible as an interpretation of the notion of "psychological tension", it is certainly more consistent with Janet's remarks concerning the nature of checking and repetition than is Pitman's "displacement" interpretation. The checking and repetition will be, on Reed's interpretation and as Janet's remarks require them to be, of the very action which the patient's "lowered psychological tension" has rendered him unable to perform to his own satisfaction.

Some of Janet's remarks thus appear to provide some support for Reed's interpretation of his work while others, as noted earlier, support the interpretation offered by Pitman. It seems, then, that neither interpretation can provide a full account of Janet's ideas and that some combination of these interpretations may come closer to this than either can alone. It is of some interest to note that, despite their contrasting accounts of Janet, both Pitman (1987[ii]) and Reed (1985) believe there to be connections between his work and cybernetic theory (see section 3.5).

3.4.5.5 Conflict and OCD

Pitman seems a little too ready to criticise Janet for supposedly underplaying the role of conflict in OCD. Pitman's position on this point is very like that of Havens (1966), both of them arguing, on the basis of the symptom contents of some OCD patients, that "intrapsychic conflict" over such matters as aggression and sex are involved. Havens, for example, presents the case (discussed by Janet) of psychasthenics who, tormented by the thought that they do not fully love their fiances, strive hard to do so and as a result of these strivings - or so Janet argued - end up detesting them. (Let us assume here that the detestation of these patients towards their fiances would sometimes take the form of <u>obsessions</u> experienced by them, thus making some of these cases instances of OCD.) This account, which presents the detestation experienced as secondary to the inability to love, may be contrasted with what Havens suggests "the contemporary mind immediately grasps" (p392) - by which he means what would be suggested by a psychodynamic approach to such problems,

within which the detestation reported would be regarded as primary and as having caused (while still unacknowledged by the patient) the inability to love. Pitman similarly remarks (1987, p228) that "the modern reader with any psychodynamic leaning" cannot fail to spot this "error" in Janet's formulation of numerous cases. Havens, noting that Janet's patients are "again and again" reported as being horrified by their symptom content, thinks it remarkable that Janet did not move towards a "conception of mental illness as conflict" (Havens, 1966, p397).

Yet it is surely not just obvious that the account Janet offers is for all cases inferior to the conflict-based one Pitman and Havens favour? Is it clearly the case, for example, that the detestation featured in Janet's example above could not have arisen in the manner he postulates? Janet is surely not just clearly wrong here. On the face of things, that is, it seems that it may be possible to explain some aggressive symptom contents as secondary to the patient's frustration at his or her doubts and inability to feel or act.

Pitman also discusses his conflict/displacement account of OCD while presenting a cybernetic model of the disorder. Some comments on his discussion there are offered below (section 3.5.3.3).

3.4.5.6 The physiological basis of displacement

It is also worth noting that Pitman is too sanguine about accepting Janet's "diversion of mental energy" or "displacement" explanation of (some) forced agitations while rejecting Janet's speculations as to the physiological basis of this hypothesised process - the interdependence of these two levels of Janet's theorising is greater than Pitman evidently supposes. This is because it is more difficult to characterise adequately the notion of a "displacement activity" when using it, as Pitman (1987[i], 1987[ii]) does, in the context of a psychological theory making no claim as to the possible physical bases of the phenomena with which the theory deals. An account which suggests that some conflict/distress is displaced in some activity must be distinguishable from accounts of that activity which argue instead that, for example, the activity provides distraction from that conflict/distress (or even, in some cases, relief from it via muscle relaxation). The observation (were it to be supplied) that some compulsive behaviour always arises in the context of, and leads to a reduction in, some conflict/distress, for example, anger the patient is unable to express (see Chapter 7), would thus not in itself be strong support for a displacement account of that compulsive behaviour. This observation would be equally consistent with the distraction (or even, in some cases, the muscle relaxation) hypothesis.

Janet's displacement activity account claims that there is some physiological

displacement corresponding to the observed "psychological displacement" and is, therefore, able to distinguish itself from these alternative hypotheses by virtue of its making this claim - these alternatives do not seem to imply, in contrast to such a displacement account, that it should be possible to observe the passage of activity from the CNS mechanisms which mediate the patient's experience of his conflict/distress to those CNS mechanisms which mediate the performance of his "displacement activity". If, therefore, Janet's speculations as to physiological processes are, as Pitman suggests, implausible, then Janet's hypothesis that psychological displacement is taking place must also be weakened, contrary to Pitman's position.

3.4.6 Janet on diagnostic and treatment issues

Janet observed that "dissociative and conversion disorders" (Pitman, 1987[i], p228) which he classified as hysterical illnesses - his other major category of neuroses - are rarely co-morbid with those symptoms which are seen in psychasthenic illness. Janet drew a number of distinctions between hysterical and psychasthenic illness (and between the people who suffer from these two illnesses). For example, the pathological ideas of the hysteric are, according to Janet, readily translated into action, perception and belief, in contrast to the pathological ideas of the psychasthenic, and whereas hysterics are usually readily hypnotizable, psychasthenics are usually the opposite. Havens (1966, p393) points out that Janet regarded hysterics and psychasthenics as being distinguishable with respect to the extent to which they are able to "hold their ideas in full consciousness". Thus, whereas the hysteric exhibits a more complete dissociation, the psychasthenic experiences only a "partial dissociation" (Havens, 1966, p393) of his ideas, according to Janet.

Heredity plays, according to Janet, an important but "not a tightly determining" role (Pitman, 1987[i], p231) in psychasthenia and he suggested that the crossing of families with vulnerability to the illness should be avoided. Janet suggested that the illness may occasionally be produced in individuals formerly in good psychological health by various stressors, but is more usually a "longlasting constitutional condition" (Pitman, 1987[i], p230) which is found in people who have always been of timid character and is exaserbated by precipitants such as losses, examinations and marriage. Psychasthenia is, Janet suggested, three times more common in women than men. The most common outcome of the illness is, according to Janet, a "relative recovery" (Pitman, 1987[i], p231) usually before the age of 40. He notes that a minority of psychasthenics develop melancholia or paranoia.

The prevention of the disorder is most readily achieved, Janet suggested, by encouraging predisposed children to "confront reality" (Pitman, 1987[i], p231). The "physical",

for example, practical activity and even fights, should be encouraged over the intellectual for such children, Janet suggests. The treatment of psychasthenia should, according to Janet, take the form of encouraging the patient not to doubt, and "to devote himself to a task and then stick it out" (Pitman 1987[i], p231). He believed that the patient should be "pushed in the direction that he really wants to go but cannot quite find it in himself to" (Pitman, 1987[i], p231).

3.4.7 Commentary on diagnostic and treatment issues

3.4.7.1 Hysterical and Psychasthenic Illness

The theorising of some Pavlovian personality theorists (for example, Claridge 1985) is once again brought to mind by Janet's distinction between dissociative and conversion disorders, on the one hand, and OCD and the disorders with which it tends to be co-morbid, on the other. Janet's suggestion that action, perception and belief are not affected in psychasthenic illness is implausible, although his observation that hysterical and psychasthenic illness involve different degrees of dissociation is intriguing. This claim may be open to numerous interpretations, but each of these meets with difficulties. For example, Havens (1966, p393), discussing the partial dissociation of psychasthenics, notes Janet's observation that at one moment the patient has full possession of his thoughts - he knows at that moment that he has not, for example, committed some crime - but then "the certainty diminishes and the ruminative doubting [i.e. as to whether he has committed the crime] resumes". But not all of the symptoms seen in OCD (and still less all of those seen in psychasthenia) involve the doubting and uncertainty with which talk of "partial dissociation" is being justified here (see section 1.3.4). Perhaps, then, the <u>insight</u> exhibited by psychasthenics as regards their pathological ideas might instead be used to characterise their disorder as involving only a partial dissociation of ideas? "Partial dissociation" would on this interpretation refer to the psychasthenic's <u>attitude towards</u> his symptoms - his agreeing with others as to their morbid nature - rather than, as in Janet's suggestion above, to doubts which are themselves <u>part of</u> the psychasthenic's symptoms. And the contrast would therefore be, on this interpretation, with the absence of insight which is observed during - and after - the occurrence of such hysterical symptoms as fugue states and somnambulisms. In reply to this, however, insight is not seen in all cases of OCD (section 1.2.6 and Chapter 2) and this feature would also be, therefore, unable to support Janet's contrast of psychasthenic and hysterical illness in terms of the degree of dissociation involved.

This contrast is perhaps most plausibly defended in terms of another of Janet's remarks, that whereas "the hysteric appears not to know what troubles her...[the psychasthenic]

knows perfectly well what torments him" (Havens 1966, p392). The psychasthenic, that is, is on this view more aware of what troubles him in that he does not exhibit amnesia with respect to his symptoms, in contrast to the amnesia exhibited by patients suffering from fugue states and somnambulisms concerning their symptoms (see Havens 1966). But, against this position, it might still be argued that not all patients suffering from hysterical symptoms are amnesic regarding them. The amnesia exhibited by patients suffering from fugue states and somnabulisms is not seen, according to this argument, in all cases of hysterical illness. If this claim is correct, a clear contrast between psychasthenic and hysterical illness has still not been drawn.

This raises the question of which symptoms Janet classified as psychasthenic, and which hysterical. There appears to be some dispute as to this. According to Pitman (1987[i], p228) Janet considered anorexia nervosa "to be a form of obsessive-compulsive behaviour" and thus a symptom of psychasthenic illness. Havens (1966), by contrast, suggests that anorexia nervosa, termed by Janet "hysterical anorexy", was classified by Janet as an hysterical disorder. It seems that anorexic patients could no more be described as amnesic with respect to their symptoms than could psychasthenic patients, and if Havens is correct as regards Janet's classification of anorexia nervosa, therefore, a clear distinction has yet to be provided between those symptoms Janet regarded as hysterical and those he regarded as psychasthenic illness, in terms of the degree of "dissociation" their sufferers exhibit. It could at most only be claimed that some hysterical symptoms can be contrasted in these terms with the symptoms seen in psychasthenia.

3.4.7.2 Empirical evidence relevant to Janet's position

Pitman suggests that Janet's comments as to the role of heredity in psychasthenia agrees with current thinking (also see section 3.1), and that Janet is also correct in claiming that while most patients eventually improve, a small percentage develop signs of psychosis. Pitman takes issue with Janet's claim of a much higher incidence of psychasthenia in women than men. Pitman's objection, however, is based on the modern evidence concerning the gender ratio for OCD and so the inclusion in psychasthenic illness of phobic and other anxiety disorders in which there is, in contrast to OCD, a large female predominance (Fineberg 1990) suggests Pitman's criticism to be unfair.

3.4.7.3 Janet and behaviour therapy

Pitman claims that the treatment recommendations of behaviour therapists were anticipated by the interventions suggested by Janet. Thus, Janet's prescription that children who are predisposed to psychasthenia should be encouraged to confront danger anticipated,

according to Pitman, the "principle of direct therapeutic exposure" while Janet's prescription that psychasthenic patients should be prevented from doubting and encouraged to persevere with tasks anticipated, Pitman further suggests, the behavioural technique of response prevention.

This appears to be an overstatement of the similarities between Janet's treatment recommendations and these behavioural interventions. The crucial ingredient of exposure and response prevention is that the patient is encouraged to confront the stimuli and tasks which feature in his symptoms, and is discouraged from carrying out the compulsive behaviour he usually performs with respect to those stimuli and tasks (see Chapter 7). This emphasis upon the patient's confronting those stimuli and that behaviour which feature in his symptoms is evidently absent from Janet's suggestion that predisposed children and patients merely be encouraged to take risks, and to persevere with tasks. Indeed, Janet's position was that "because obsessions are symbolic expressions of an underlying state, treatment must be directed not at the symbol but at the state itself" (Pitman 1987[i], p231). This would, if anything, appear to be an anti-exposure position.

There may be other behavioural approaches to therapy which have more in common with Janet's recommendations. Emmelkamp and van der Heyden (1980) note, like Janet, the timidity of some OCD patients and attempt to use this observation both to help make sense of, and to treat, some kinds of OCD symptom. Thus, Emmelkamp and van der Heyden suggest that "harming obsessions" - obsessions which feature the theme of hurting other people or oneself - result from unexpressed aggressive feelings which in turn result, according to Emmelkamp and van der Heyden, from the inappropriately unassertive behaviour of those patients who report such obsessions. Emmelkamp and van der Heyden report that the use of assertion training with these patients led to both an increase in assertive behaviour and a reduction in harming obsessions. This study is discussed at some length below (Chapter 7), as is a case discussion of Malan's (1979) which, from a psychodynamic perspective, adopts a somewhat similar therapeutic approach (see section 3.6).

Janet's suggestion that children who are predisposed to psychasthenia should be encouraged not to avoid confrontations particularly calls to mind Emmelkamp and van der Heyden's study. Janet's "anti-exposure" remarks quoted above may also be interpreted so as to be consistent with Emmelkamp and van der Heyden's therapeutic approach. Thus, to the extent that "harming obsessions" may be seen as being a "symbol" of the "underlying state" of the patient - as being a "symbol", that is, of the timidity and unassertiveness of these patients - assertiveness training follows Janet's prescription. Rather than aiming treatment

directly at this "symbol" - as would be done, for example, in exposure therapy - assertiveness training addresses itself instead to the "underlying state" of these patients (although clearly Emmelkamp and van der Heyden would be best described as regarding "harming obsessions" as a product or reflection, rather than a symbol, of unassertiveness).

The account of "harming obsessions" offered by Emmelkamp and van der Heyden is very like that which Havens (1966) and Pitman (1987[i]) would put forward. Emmelkamp and van der Heyden suggest that feelings of guilt concerning aggressive feelings and the expression of such feelings ("conflict", in Haven's and Pitman's terms, over such feelings and their expression) produce the patient's unassertive behaviour. Does, then, the successful use of assertion training with these patients, introduced by Emmelkamp and van der Heyden on the basis of their account of harming obsessions, favour Havens' (1966) and Pitman's (1987[i]) explanation (see sections 3.4.5.4 and 3.4.5.5) of the genesis of such symptoms over Janet's? It would appear not. It seems that Janet's account of the harming obsessions of Emmelkamp and van der Heyden's patients would not have been very different from that offered by these authors, or by Pitman and Havens. Evidently, Janet too would have suggested these obsessions to be an effect of the timidity of these patients, and that their acting in a more assertive manner would have reduced the severity of their symptoms. Perhaps Janet would have assigned guilt a less important role in the distress of these patients than that which it is given by Emmelkamp and van der Heyden? But even if this is true, this part of Emmelkamp and van der Heyden's account plays no part in formulating their prediction that assertiveness training should help patients with harming obsessions, and may even indeed be incompatible with this prediction (see Chapter 7).

The situation would evidently be different if assertiveness training were shown to be effective in helping those patients of Janet's who reported feelings of detestation towards their fiances - or at least, if assertiveness training could be shown to be effective in reducing the doubts these patients have concerning their love for their fiances and/or their inability to feel love for them. Janet's account suggests in such cases that it is because of the patient's inability to feel or express <u>tender</u> emotions that she experiences feelings of detestation, and these patients may therefore be contrasted in this respect to those treated by Emmelkamp and van der Heyden - their symptoms were hypothesised by these authors to result from a failure to express anger. Encouraging these patients of Janet's to express hostile emotion should not, therefore, on Janet's account, make any difference to their difficulties in experiencing or expressing tender emotions. The patient's hostile emotion is on this account a mere effect of these difficulties and its expression would evidently not be predicted, therefore, to be of help

in alleviating them.

This prediction distinguishes Janet's account from one which is at least very like that which would be defended by Havens (1966) and Pitman (1987[i]). They suggest that the patient's detestation for her fiance produces her doubts about, and her inability to experience fully, her love for him. If this detestation were in turn hypothesised to be a result of the patient's inability to express hostile emotion appropriately - her not, that is, behaving in a sufficiently assertive manner - then it would be predicted that an intervention which attempted to promote more assertive behaviour might help alleviate the patient's difficulties in experiencing or being sure about tender feelings.

Janet's account may also be distinguished from one which claims that the primary difficulty of patients such as those treated by Emmelkamp and van der Heyden is that these patients are abnormally angry, their timidity and unassertive manner being explained as their reaction to - their compensation for - this abnormal degree of anger. Such an account, in contrast to Janet's, seems to be contradicted by the results of Emmelkamp and van der Heyden's study. Emmelkamp and van der Heyden have provided some evidence in favour of the claim that adopting ordinary levels of assertiveness is therapeutic for patients with harming obsessions (see Chapter 7). An account of these symptoms in terms of the patients having an abnormal level of anger appears to predict that similarly abnormal levels of assertiveness or aggression would be necessary for a significant reduction in the severity and frequency of obsessions.

3.4.8 Summary

Pitman praises the "clinical astuteness" (Pitman 1987[i], p231) of Janet's work, and suggests that it is not so much Janet's theory of psychasthenia but rather his observations and treatment suggestions concerning the disorder which have stood the test of time. Janet's theory of psychological tension and his speculations as to a "physiological tension" corresponding to it, were, according to Pitman, an unsuccessful attempt to make sense of Janet's observations concerning psychasthenic patients. Some of these observations, however, such as those concerning the interpersonal difficulties encountered by these patients and the co-morbidity of the various symptoms they report, remain important contributions. In the light of Reed's interpretation it needs to be added that opinions differ as to how Janet's theory of psychasthenia is to be understood, but this turns out not to be a reason for questioning Pitman's lack of confidence in the theory - Reed's own account of OCD which has, as noted earlier, much in common with his interpretation of Janet, meets with substantial difficulties (Chapter 4). It is of interest that some of Pitman's suggestions as to the nature of OCD, made

while outlining a "cybernetic model" of the disorder, have some similarities with Reed's account. It has also been noted that the account of some symptom contents to which Janet was led by his theory of psychological tension may have been too readily rejected by Pitman and Havens. Something like their psychodynamic accounts, on the one hand, and Janet's account, on the other, may provide contrasting predictions as to what is likely to be the outcome of using assertion training with some (but not all) of the psychasthenic patients described by Janet.

As regards the treatment of OCD, doubts have been raised as to the extent to which Janet's suggestions amount of the recommendation to use exposure with response prevention. Concerning Janet's clinical observations, doubts have also been raised as to the extent to which these would apply to all OCD patients. Nonetheless, some of the observations which Janet provides may well be of importance in making sense of, and even in providing treatments for, some cases of OCD.

3.5 Pitman's Cybernetic Model of OCD

3.5.1 The principle of a control system

Pitman (1987[ii] - unless otherwise stated all future references are to this article) subjects "aspects of obsessive-compulsive disorder...and related psychopathology to a cybernetic, or control systems, analysis" doing so in the light of Janet's (1903) observation that OCD patients experience their mental activity as "incomplete" (see section 3.4.2). Pitman suggests (p334) that his analysis of OCD in cybernetic terms, a component of modern neuroscience unavailable to Janet, will compliment Janet's account of the disorder. As noted above (section 3.4.5.4), Reed (1985, p176-7) also discusses the relevance of cybernetic theory to Janet's account of OCD.

Central to the cybernetic approach to behaviour, Pitman points out, is its claim that the "organism constitutes a complex control system which attempts to control not its behavioural output...but rather its input or perception, through a negative feedback process" (p334). He illustrates the principle of a control system with the example of a thermostat. An "internal comparator" mechanism detects mismatch between a "perceptual signal" - the ambient temperature reading - and a "reference signal" - the thermostat's setting - and generates an "error signal". This causes the activation of "behavioural output" - the turning on of the heating system - in order to shift the perceptual signal in the direction of the reference signal. This continues until the mismatch between the perceptual and reference signals is eliminated, at which point the error signal becomes zero and the behavioural output stops.

The behaviour of a human being may be similarly understood, Pitman suggests. He

gives as an example a person adjusting a thermostat - this behaviour may be seen, Pitman suggests, as resulting from the person's "comparator" calculating the difference between "an internal comfort perceptual signal and an internal comfort reference signal". This leads to the generation of "an internal error signal, which represents [the person's] discomfort" (p335). This causes the person to adjust the thermostat until his discomfort - "internal error signal" - is eliminated. Pitman notes that different control systems within the same individual may conflict. This is so when a person wants two incompatible goals; in such circumstances, it will be impossible for that person's "error signals" regarding both of these goals to be simultaneously at zero.

3.5.2 Pitman's general remarks regarding the Cybernetic Model

Pitman's "cybernetic model" of OCD actually consists of what he presents as three different suggestions as to the nature of this disorder. Before introducing these suggestions, Pitman offers some remarks concerning the cybernetic approach to behaviour in general. Pitman suggests that psychological theories in general, and behavioural theories rooted in traditional stimulus-response psychology in particular, have only had limited success in explaining OCD because, "uninformed by cybernetics" (p335), they do not consider "the systematic interaction of perception, purpose and overt action" (p335) which characterises all behaviour, including that observed in OCD. Pitman considers that control systems theory, by contrast, "offers unique explanatory possibilities for OCD" (p336). He hypothesises that "the core problem in OCD is the persistence of high error signals, or mismatch, that cannot be reduced to zero through behavioural output" (p336) and presents a number of examples of OCD symptoms which, he argues, may be understood in this way, including perfectionism and indecision.

Pitman proposes that this model could be tested experimentally by tasks requiring OCD patients and controls to report the degree of mismatch they perceive in different classes (for example, auditory, visual, tactile) of paired stimuli. The model predicts, Pitman suggests, that OCD patients will be more inclined than controls to detect mismatch between pairs of stimuli.

Pitman adds to his analysis some speculations as to how it might be applied to current neuroanatomical hypotheses of OCD. Among other contributions, he notes that Gray (1982) has suggested a role for the septo-hippocampal system and associated limbic areas in OCD, postulating that these areas function to compare predicted to actual events, taking control of behavioural output when mismatch is detected. Pitman finds this analysis similar to his own, although he notes that the emphasis in Gray's work is upon predictions or expectations rather

than, as in Pitman's analysis, intentions, as reference signals for behaviour (see section 3.3.6.1).

Before considering Pitman's three suggestions regarding OCD, let it first be noted that many of the claims of the cybernetic approach, as presented by Pitman, appear to be entirely uncontroversial. This approach, for example, as Pitman emphasises, conceives of behaviour as purposeful, as being performed in order to bring about certain desired states of affairs, rather than, in contrast to "S-R psychology", as "simply...learned or innate responses to instigating stimuli" (p335). This is, indeed, what Pitman's suggestion that "perception, purpose and overt action interact in all behaviour" really amounts to. But this claim is not, therefore, one which would be denied as regards the compulsive behaviour of OCD patients, even by behavioural theorists of the disorder such as, for example, Rachman and Hodgson (1980). Similarly, the suggestion that error signals or the perception of mismatch produce compulsive behaviour is not in itself substantive, evidently only amounting to the claim that this behaviour is produced by thoughts such as, for example, "X is not perfectly clean" and any discomfort which the patient experiences in association with such thoughts.

3.5.3 The three accounts presented by Pitman

3.5.3.1 The "faulty internal comparator" account

Pitman suggests that in the case of some OCD patients, difficulty results from a "faulty internal comparator" - no matter what perceptual input [the patient's comparator] receives", Pitman suggests, "it continues to register mismatch" (p340) and thus "while the obsessive-compulsive often feels that an action wasn't done well or completely, to an observer it may appear perfectly well done".

These remarks are open to different interpretations. There are thus at least two quite different faults which an OCD patient's "internal comparator" could be hypothesised to have developed, these being that the "error signals" generated by the comparator are (i) exceptionally difficult to extinguish or (ii) exceptionally strong (it is unclear which of these interpretations Pitman would accept). These two hypotheses need to be carefully distinguished. Hypothesis (i) differs from most other accounts of OCD - including hypothesis (ii) - by the fact that it regards as primary the difficulty the patient has in dismissing, for example, thoughts that checking or cleaning tasks have not been properly done. Hypothesis (i) would suggest that it is this difficulty which causes the distress observed, this being held by this approach to be in particular not a matter of the thoughts of not having properly cleaned, or of having made a mistake, being more aversive for the OCD patient than they are for others. Thus, hypothesis (i) would, for example, deny that it matters more to the OCD patient whether

or not he has been contaminated or made a mistake - it is rather, according to this hypothesis, only that he finds it more difficult to establish whether cleaning or checking tasks have been properly performed. This is what is meant here by the "faulty comparator" producing signals which are difficult to extinguish, as opposed to being exceptionally strong.

This hypothesis has some similarities to that put forward by Reed (1985, 1990) (see Chapter 4) (although Pitman makes, in contrast to Reed, some attempt to explain obsessional impulses - see Pitman, p338 and section 4.6.1 below). According to Reed, obsessional disorders are primarily cognitive in nature, the thinking style of the patient being hypothesised by this approach to produce the patient's discomfort.

A number of difficulties raised for Reed's account below (see Chapter 4) may also be brought against hypothesis (i). Two of the most important of these difficulties will be mentioned here. Firstly, there have been numerous studies which have used tasks very like those suggested by Pitman in which the "degree of mismatch" perceived by OCD patients has been tested, and these studies have failed to substantiate hypothesis (i) (see section 4.8 and Part C below) (although see section 3.5.3.3 for a reply Pitman might make at this point).

Secondly, as stressed above, hypothesis (i) implies, with Reed's account, that the outcomes of, for example, cleaning or checking tasks should not matter more to the OCD patient than they do to others - the OCD patient differs from other people, according to hypothesis (i), only in being less able to establish that satisfactory outcomes to such tasks have been achieved. Yet this implication is implausible. Many normals, for example, are able to tolerate the possibility, and even indeed the certainty, that their hands have not been properly cleaned or that doors and switches etc. have not been properly checked. It cannot, therefore, merely be the hypothesised difficulty of the OCD patient in establishing that such tasks have been properly done which is the cause of his difficulties on these tasks.

The distress and compulsive behaviour exhibited by such a patient can only be explained by a <u>greater intolerance</u> of the thought that his hands may not be or are not properly washed, and that doors and switches etc. may not be or are not properly checked, and no account of this greater intolerance is provided by hypothesis (i). This account fails to explain the <u>motivation</u> of OCD patients (see section 4.3 for a fuller statement of this objection).

This difficulty in explaining the motivation of OCD patients is not encountered by hypothesis (ii), if the "exceptionally strong" error signals postulated by this hypothesis are understood to correspond to the OCD patient's experiencing more anxiety (or some other discomfort) at the thought that, for example, his hands have not been properly washed, or doors and switches etc. not properly checked. But an account of OCD must surely tell us <u>why</u>,

not merely that, this is so. And there appears to be nothing in Pitman's "faulty internal comparator" discussion which attempts to meet this requirement.

A further point is worth noting. As pointed out above, Pitman suggests that his account to some extent overlaps with Gray's, and this claim seems most likely to have been made with the "faulty internal comparator" section of Pitman's discussion in mind. But the interpretation of that section of his discussion offered as hypothesis (i) above is readily distinguishable from Gray's account due to its implying, as noted above, that the outcomes of, for example, cleaning and checking tasks should not matter more to OCD patients than to others. While encountering difficulties of its own (see section 3.3.6), Gray's account is clear that OCD patients should experience more anxiety than others regarding the tasks which trouble them, and therefore has no problem explaining why these tasks matter more to these patients. If hypothesis (i) is, therefore, a correct interpretation of Pitman's remarks, his account must evidently have less in common with Gray's than Pitman believes.

3.5.3.2 The "attentional disturbance" account

Pitman suggests that in other OCD patients there may be an "attentional disturbance", involving a "diminished capacity to withdraw attention from...discrepant perceptual signals" (p340). Thus stated, however, it is at best unclear how this difficulty could be distinguished from the patient's having a "faulty internal comparator" the error signals from which are either difficult to extinguish or exceptionally strong. Perhaps the claim would be that the patient's "attentional disturbance" will also be evident elsewhere in his functioning. But Pitman does not attempt to fill out his suggestion in this or any other way, and so for the purposes of the present discussion the "attentional disturbance" and "faulty internal comparator" hypotheses will be treated as indistinguishable.

3.5.3.3 The conflict/displacement account

Pitman is explicit that he does not intend the "internal comparator defect" or "attentional disturbance" accounts to be the usual ways in which he intends his cybernetic approach to be applied. The application he most favours is within a conflict/displacement account, this being the account which provides, Pitman believes, the most plausible explanation of at least most cases of OCD. Cybernetics can help illuminate such an account, Pitman argues, by suggesting that conflict results from two control systems having different reference signals for the same input or perception. One example Pitman gives is of a patient wanting both to confront and not to confront his boss. The problem is thus a matter of "intrapsychic conflict" (p339), with compulsive rituals being hypothesised to represent the displacement activity which results from this conflict (also see sections 3.4.5.4 and 3.4.5.5).

But how significant a contribution can cybernetic theory make within such an account as this? Neither the notion of "intrapsychic conflict" nor that of "displacement" is supposed by Pitman to have been introduced by cybernetics. He finds the notion of "displacement" in the work of Janet to whom, as noted earlier, Pitman himself points out cybernetics was unavailable and he finds (Pitman, 1987[i], p228) both the notion of "displacement", and that of "intrapsychic conflict", in the work of Freud, to whom cybernetics was similarly unavailable. All that remains, therefore, for the cybernetic approach to contribute within this account is the observation that the competing behaviours involved in the putative conflict are purposeful. But this appears to be an entirely uncontroversial claim. All theorists would accept that <u>if</u> conflict of the type Pitman is discussing here is involved in producing OCD, then such conflict will be between purposes the patient has. Cybernetics, it may be concluded, cannot be making any important contribution to this conflict/displacement account of OCD.

What, then, of this conflict/displacement account itself, and in particular the suggestion that "intrapsychic conflict" causes some cases of OCD (also see section 3.4.5.5)? This suggestion is not supported by the cases which Pitman (1987[ii]) cites as illustrating the role conflict plays in OCD. He presents, for example, the case of a patient who experienced difficulty in choosing between two desired options on a menu. Unable to decide which of the options he wanted most, the patient would, Pitman tells us, vacillate between them for prolonged periods of time. But this does not show conflict to have been involved in <u>causing</u> such a symptom in the manner required by Pitman's conflict/displacement account, or show that this patient's indecisiveness is a <u>displacement</u> of any such putative conflict. The conflict to which reference is being made here is <u>part of</u> the symptom, and it is thus just a description of the problem, rather than a causal claim, to say that the patient's difficulties with menus involves conflict of this kind. Similarly, nothing here suggests that the conflict involved has produced any "behavioural pattern...that cannot be attributed to either competing drive" (see section 3.4.5.5), the suggested role for conflict in Pitman's conflict/displacement account. And it seems, therefore, that once again Pitman's cybernetic approach cannot tell us anything further about problems such as this patient's difficulties with menus except for the fact that such a problem involves the patient's harbouring competing desires or intentions. And yet this much is, once again, surely already evident merely from a description of the patients difficulties?

As regards those cases in which Pitman hypothesises the conflict/displacement account to apply, he is explicit that "one need not postulate structural abnormality with either control system [that is, those which are hypothesised to be in conflict]". His position is thus explicitly

that there is only an intrinsic comparator defect in some cases of OCD, which appears to require some qualification of his prediction (p339) that it should be possible to show that OCD patients differ from controls when asked to report their perceived degree of mismatch concerning paired stimuli in laboratory tasks. This is especially so as Pitman introduces his hypothesis of an intrinsic comparator defect only after stating that "it is tempting to hypothesize that all OC psychopathology is caused by underlying conflict" (p340), but then conceding that "a causative role for conflict is difficult to establish in all cases" (p340). It seems that Pitman's prediction as regards the performance of OCD patients on the laboratory tasks he proposes would, according to these remarks, only apply to a minority of such patients, and would thus make it possible for him to explain experimental findings such as those referred to in section 3.5.3.1.

Another problem for the conflict/displacement account is that it is at best unclear that it would predict difficulties in matching perceptual and reference signals - difficulties in establishing that, for example, cleaning or checking tasks have been properly done - to be evident in compulsive behaviour at all. According to this account, difficulties in matching perceptual and reference signals arise as a result of conflict being encountered elsewhere, producing compulsive behaviour as a displacement activity. Would the account not predict, therefore, that if severe difficulties in matching perceptual and reference signals are also encountered in the area in which the displacement activity arises, a second displacement should occur in response to these difficulties, and so on until an activity not involving such difficulties is found? If the point of some activity, that is, is supposed to be the attempted relief or resolution of difficulties in matching perceptual and reference signals encountered elsewhere in the patient's functioning, it seems that one would not expect this activity itself to involve such matching difficulties. If this is correct, then all of the examples Pitman gives of compulsive behaviour involving difficulties in matching perceptual and reference signals may actually contradict the conflict/displacement account. (This, incidently, suggests an interesting contrast between accounts of compulsive behaviour in terms, on the one hand, of displacement mechanisms and, on the other, in terms of symbolic mechanisms - at least some versions of the latter type of account evidently would predict that compulsive behaviour should reflect the conflict experienced by the patient [symbolisation is further discussed in section 3.6.4.3].)

3.5.4 Summary

In presenting a cybernetic model of OCD, Pitman discusses a number of interesting cases where symptoms involve indecisiveness, perfectionism and conflict. But, insofar as the

cybernetic model makes substantive suggestions as to the explanation of OCD - in terms of a fault in the postulated "internal comparator mechanism" making error signals more difficult to extinguish - it encounters a number of difficulties, the most important of these being in making sense of the motivation of patients suffering from the disorder. Insofar as these difficulties are avoided - by supposing the internal comparator fault to produce error signals which are, in contrast, abnormally strong, or by presenting the cybernetic model within a conflict/displacement account - it appears that the cybernetic component of these accounts fail to make any substantive contribution to them. In its present form, therefore, it must be concluded that the model does not significantly advance our understanding of OCD.

3.6 A psychodynamic approach to OCD

3.6.1 Introduction

The question of whether or not psychoanalysis can be refuted, and the closely related question concerning whether or not psychoanalysis may be regarded as a science, continue to be a matter for debate. There will be in what follows no attempt to examine these questions in any detail, although one or two brief concluding remarks concerning these matters will be offered. There will also be no attempt in what follows to examine the whole of the psychodynamic literature on OCD. The discussion will instead mainly focus on a case presented by Malan (1979) and used by him to discuss the topic of OCD in general. It is hoped that a close examination of Malan's arguments regarding this case will be of some use in evaluating psychodynamic approaches to OCD in general.

3.6.2 Malan's general approach

3.6.2.1 Developmental stages

As Malan (1979, p183) points out, Freud suggested that "the child's "sexuality" passes through three phases, being mediated first through the mouth, then the anus and finally the genitals - the oral, anal and genital phases". According to Freud (1895, 1913), later elaborated in Fenichel's (1945) aphoristic account, fixation at, or regression to, a particular stage of psychosexual development determines the nature of the disorder which appears in later life. In the case of OCD, the putative stage of development involved is the anal stage. Toilet training is supposed to be of central importance to this stage and anger and aggression to be associated with it. Fixation at, or regression to, this stage of development is also argued by Freud to determine the development of the obsessional personality and personality disorders.

The absence of evidence in support of theorising of this kind has been frequently noted, for example, de Silva (1988). Malan (1979) for the most part accepts that such theorising lacks evidential support and is inclined to reject accounts of human development

which, like Freud's, stress the importance of "parts of the body" (Malan, 1979, p185). Malan further suggests that much of the psychodynamic literature makes an error which he terms "psychological anachronism", this being the attributing to an infant phenomena that really belong to a much later and more sophisticated stage of development. Malan is able to make these points while still presenting what is, as will be argued below, a recognisably psychodynamic account of OCD.

3.6.2.2 Psychodynamic therapy

According to Malan, a psychodynamic explanation should (a) indicate how that the factors which precipitate a given symptom suggest the putative conflict which is supposed to be expressed or represented by that symptom and (b) provide a "detailed mechanism" whereby this expression or representation has occurred. Interpreting this mechanism to the patient should (c) "bring the conflict clearly into consciousness" and this should (d) "result in the disappearance of the symptom" (Malan, 1979, p107, emphasis added). Malan thus argues that therapeutic change in psychodynamic therapy directly results from the bringing of conflict into consciousness. Malan suggests that conditions (a)-(d) are similar to "Koch's Postulates" by which one can judge whether a given disease is caused by a particular bacterium (Malan, 1979, p107).

Malan also acknowledges (1979, p218-9) that psychodynamic therapies have had little success with OCD and argues, contrary to his account of psychodynamic therapy, that when "obsessional symptoms, and particularly obsessional rituals" are treated by psychotherapy, "...everything becomes intelligible and the patient becomes conscious of the conflict, but therapeutic results do not ensue "(Malan, 1979, p107). Malan argues that (p107) a major challenge for psychodynamic theories of OCD is the explanation of what he describes as the positive effects of behaviour therapy with this disorder.

Not all psychodynamic theorists would accept Malan's account of psychodynamic therapy and in particular his suggestion that therapeutic effects are the direct result of bringing conflicts into consciousness. Syminginton, for example, would regard such an account as giving "too much weight to the act of insight"(Syminginton 1985, p26) and suggests that this picture is contrary to the whole psychodynamic conception of Man as "ruled by the irrational"(1985, p26). He instead suggests insight to be an effect of change in psychotherapy and argues that "emotional change and insight are manifestations of things going on at a deeper level of the psyche" (Syminginton 1985, p36).

3.6.2.3 Evidence relevant to psychodynamic formulations

Malan presents (1979, p55) "four categories of evidence" as providing potential

support for any psychodynamic formulation. The first three of his categories restate the four conditions Malan discusses as similar to "Koch's postulates", and Malan adds to them another possible line of evidence which he describes as "the patient's response to interpretations". By this he means whether or not the interpretation deepens the rapport between the patient and therapist. There is an asymmetry in Malan's discussion of this point. He suggests that an interpretation may be "inappropriate" - that is, reduce rapport - while being perfectly true. This will be so, for example, if the patient is not ready to accept the interpretation, Malan suggests. But this raises the question of whether or not false interpretations may sometimes deepen rapport because they happen to be, for example, merely what the patient wants to hear. Malan does not discuss this point but does make frequent use, in his case discussions, of an interpretation's deepening rapport as confirmatory evidence for that interpretation. He evidently holds, therefore, that it is at least very unlikely that false interpretations could have this effect.

3.6.2.4 The "two triangles"

Malan divides interpretations into two kinds, those concerning what he calls the "triangle of conflict" and those concerning what he calls the "triangle of person". Malan suggests that almost every interpretation can be presented in terms of one or both of these two "triangles".

Malan describes the "triangle of conflict" as consisting of the patient's "defence", "anxiety" and "hidden feeling". Examples of each of these will be provided in a case discussion below. The "triangle of person" consists of what Malan calls "other", "transference" and "parent". ("Other" is usually used to refer to relationships with people in the patient's current life or recent past - such as a marriage partner or close friend - while "transference" refers to the relationship between the patient and therapist.) It is in his relationships with people in all three of these categories that the patient's triangle of conflict is supposed to occur. The same "hidden feeling", "defence" and "anxiety", that is, should be at work in all of these relationships, Malan suggests, and the aim of psychotherapy is, according to Malan, to help the patient trace them back from the present - the "other" and "transference" relationships - to the past - the relationship with the parent(s). The feelings that were directed towards the parent(s) are supposed by Malan to be the reason for the feelings which are found in the present relationships. The feelings in these present relationships are thus seen as co-effects of this common cause.

Elsewhere, Malan (1976) presents statistical evidence which suggests that those therapies in which what are supposed to be the patient's hidden feelings regarding his or her

parents are reached are those which also tend to be the most successful. Further evidence would, of course, be required to show that it is because of this that these therapies are successful.

3.6.3 The case of the "pesticide chemist"

3.6.3.1 Malan's account of the "pesticide chemist"

This case is Malan's (1979) most detailed discussion of an obsessional problem. Malan hypothesises in this case that the problem results from unexpressed anger but he is explicit that he does not intend this case to be seen as typical of all instances of OCD - he says: "I would not wish to give the impression that obsessional symptoms are always a defence against aggressive feelings...[they] can be a defence against any kind of disturbing conflict" (Malan 1979, p109).

Malan is certainly correct that one sees cases of OCD quite unlike that of the "pesticide chemist". It is worth noting in passing here, for example, that in his account of so-called "primitive phenomena" (Malan 1979, Chapter 15) Malan discusses the theme of "attacking the good" with respect to a patient (not suffering from OCD) whom he describes as endeavouring to destroy "everything good of his own" (Malan 1979, original emphasis).This certainly seems to share something with a phenomenon one observes in some cases of OCD - a tendency for symptoms to arise in, and to wreck, situations and activities which would otherwise be a source of great pleasure for the patient. Such features as these seem less readily observed in the case of the "pesticide chemist".

The "pesticide chemist" is described by Malan (1979, p101-7) as an over-conscientious person suffering from what Malan describes as three "obsessional symptoms": "(i) perfectionism, which he finds it difficult to maintain e.g. in relation to the assistants working under him; (ii) a need to keep things tidy, which he also cannot maintain; and (iii) obsessional anxiety, in the form of a preoccupation with the fear that he may not have done his work properly..." (Malan 1979, p102).Despite his efforts at work, the patient is reported not to have received much appreciation from his boss, and also to have clashed with his wife over the amount of time he spends at work. He is further described as always having suffered from premature ejaculation and to have been mildly depressed for the few months prior to referral.

Things came to a head, Malan reports, when the patient's wife suggested one morning that he spend a day at home due to his low mood. He chose instead to set off for work, at which his wife said that she would in that case get on with treating the house for woodworm. This was a job which he had been working on himself for a year or so, and he subsequently reported to his therapist that he took this remark of his wife's as an implicit criticism of his

efforts in this regard. His response was an uncharacteristic outburst of rage, during which he tipped the anti-woodworm solution down the sink, struck his wife and finally cried for some hours.

It is worth noting here that this patient may not, contrary to Malan's account, be suffering from obsessional symptoms at all. He is reported to have sought help regarding only his loss of energy and depression, and his outburst of rage. What are described as his "symptoms" may thus be at most only the operation of obsessional traits.

3.6.3.2 The "triangle of conflict" for the "pesticide chemist"

Malan points out that, with respect both to his boss and wife, the patient reported that he encountered his best efforts being met with criticism. Malan also notes that running through the patient's three "symptoms" is the "theme of precariously keeping something at bay" (1979, p102, original emphasis), that is the patient's own and other's potential mistakes. Malan brings these pieces of evidence together and adds to them a third - that when the patient does breakdown, this takes the form of an intense outburst of rage.

In the light of these points, Malan raises the question of whether "all three of these obsessional symptoms are symbolic ways of controlling angry feelings?" (p102, original emphasis). This, then, gives us the triangle of conflict for this patient: "...the defence is symbolic control, while the anxiety is his fear of the harm he may do if he expresses the hidden feeling, which is anger" (Malan 1979, p102-3, original emphasis).

The suggestion that the demands which others are placing upon this patient may be of importance to understanding his problems seems to be given some support by the subsequent developments in his therapy. His hypothesised anger at these demands is "brought into consciousness", Malan tells us, with the result that the patient is able to resist the demands which are being made upon him and become angry with people when necessary. His obsessional "symptoms" are reported to have disappeared at two month follow-up, this situation being maintained at a further follow-up, nearly four years later.

This does obviously not amount to experimentally controlled evidence that encouraging this patient to resist the demands of others has been a crucial ingredient to his therapy. Controlled evidence that such an intervention may be helpful for some OCD patients is provided, however, by Emmelkamp and van der Heyden (1980) (see section 3.4.7.3 and Chapter 7 for a discussion).

3.6.3.3 The "triangle of person" for the "pesticide chemist"

Turning to the triangle of person, Malan asks where the anger, which he suggests to be apparent in this patient's various current relationships, has originated and suggests that it

all goes back to this patient's parents, and in particular his father. It seems that his father was constantly away from home earning money for the family, rendering him someone "with whom it was impossible to be angry" (Malan 1979, p103) despite the father's lengthy and sorely-felt absences. It is this, Malan suggests, "which is likely to have set up a conflict in him [the patient] about whether he should express his anger and make demands on his own behalf, or control his anger and meet the demands of his environment".

3.6.4 The psychodynamic nature of this case discussion

3.6.4.1 Introduction

Malan's account of the "pesticide chemist's" difficulties is recognisably psychodynamic despite its not containing any theorising as to psychosexual developmental stages or infancy experiences etc. What is it, then, which gives this account its psychodynamic character? There are at least four features which might argued to give it this character - its referring to "unconscious" mental processes and "symbolic" mechanisms, its explanation of the patient's difficulties in terms of his purposes and its suggestion that insight is central to the treatment of the patient's difficulties. Each of these features will now be considered in turn.

3.6.4.2 The "unconscious"

There is much talk in Malan's account of there being unconscious emotions at work, specifically anger and the anxiety which is hypothesised to be provoked by it, and it might be thought that this talk helps to characterise his account as psychodynamic. But all that such talk need amount to is the claim that the patient is not in perfect touch with his own emotions or, put in other words, is not fully aware of these emotions. There is, therefore, no need to reify "the unconscious" in order to make sense of this talk. Malan is arguing both that the patient is angry and that he is not aware of his own anger, and that the patient is anxious, but wrongly believes this anxiety to have been caused by tasks at home and at work when in reality he is anxious about being angry. Couching these claims in terms of the patient "not being in touch with" or "not fully aware of" his own emotions is not only to state his putative problems in a different way. It also leads one very readily to think of there being degrees to which the patient may be unaware of his own emotions. Dichotomous talk of conscious and unconscious processes might perhaps be thought to carry the contrary implication, an implication which would, despite their using this terminology, probably be unwelcome to writers like Malan.

More importantly, the claims that people may not be in touch with their own feelings, and that this may sometimes lead them into difficulties, seem to be pieces of common sense (also see Ryle 1949). If there is, then, anything distinctively psychodynamic about Malan's

talk of "unconscious" processes, this is at most a semantic matter. This is not in itself, of course, confirmation of Malan's formulation of the "pesticide chemist's" problems. But if this formulation is to be rejected, this cannot be on the grounds that "the unconscious does not exist " when "the unconscious" is understood in the terms outlined above.

3.6.4.3 Symbolisation

Malan makes use of the notions of "symbol", "symbolic control" etc. in his discussion of the case of the "pesticide chemist". He argues that the feature of "being precariously kept at bay" is shared by both the patient's anger on the one hand, and the task demands he perceives himself to face at home and work on the other, and it is this similarity which Malan cites in claiming that these perceived task demands symbolise the patient's anger for him, rendering his attempts to meet these perceived task demands symbolic ways of controlling angry feelings. This similarity can surely not, however, be in itself sufficient to justify the claim that perceived task demands symbolise the patient's anger, and it is perhaps not entirely clear on what grounds Malan himself thinks this claim to be justified. Other than there being such a similarity in terms of which one thing may be said to "symbolise" another, then, what else is implied by talk of "symbolisation" ,"symbolic control" etc. in the present (that is, psychiatric) context? It might be argued that another thing which this implies is that, to take the example of the "pesticide chemist", this patient's hypothesised anxiety as regards his own anger has in some sense become his anxiety about perceived task demands and his hypothesised anger has in some sense similarly become his perception of those task demands. If talk of "symbolisation" is to be justified, that is, it is not sufficient, according to this argument, that the patient should have merely mistakenly attributed his anxiety about one thing (his anger) to something else which is in some respect similar to it (perceived task demands). It must, rather, be the case that his anxiety as regards that which is being symbolised (anxiety about his anger) has turned into anxiety about that by which it is symbolised (anxiety about perceived task demands), and similarly that which is being symbolised (anger) has turned into that by which it is symbolised (perceptions of task demands).

It is worth noting that the hypothesised process of converting emotion about one thing into emotion about something else, and the hypothesised process of converting emotion into perceptions of tasks demands, look as if they might be of some use or help to the patient, in contrast to the process of merely wrongly attributing emotion. These hypothesised processes of converting emotion look, that is, as if they would enable the "pesticide chemist" to gain some control over his emotions by meeting the task demands he perceives.

Does the foregoing argument imply that there is some tension between Malan's talk

of this patient's anger being "symbolised", on the one hand, and his talk of the patient's anger being "unconscious", on the other? It will be recalled that talk of the patient's anger being "unconscious" was suggested above to mean that "the patient is angry, but does not realise that this is so". Yet, after the hypothesised symbolisation has taken place, is it not, on the above account of "symbolisation", correct to say that the patient is no longer angry at all? And that he can thus not be unconsciously angry, on the above account of "the unconscious"? Yet it seems paradoxical, from a psychodynamic point of view, to say that an emotion cannot be both unconscious and symbolised at the same time. Perhaps, however, the present discussion does not make it necessary to say this? After all, it is still the case, on the present interpretation of Malan's remarks, that the explanation of the patient's becoming concerned about task demands is that he had been angry, and this anger may thus still be said to be what in some sense his concern for task demands is "really about".

To say that Malan's use of the term "symbolisation" implies these processes of "converting" emotion to be taking place is, of course, not to say that Malan provides any good reason for supposing these processes to be taking place (or indeed for supposing that they ever take place). Why, then, should one suppose that they are taking place in the case of the "pesticide chemist?" At least two quite independent doubts as to Malan's position seem plausible.

Firstly, as mentioned above, Malan lays stress upon there being a similarity between the hypothesised precipitants of the patient's symptoms and the symptoms themselves. Yet there will always be <u>some</u> similarity between <u>any</u> precipitating stress or situation, and <u>any</u> symptom, a fact to which some philosophers of science, most notably Popper (1972), have drawn attention in criticising psychoanalytic work. If "symbolic meaning" can in this way "explain" <u>any</u> symptomatic response to <u>any</u> stress, then it is evidently not explaining anything at all.

Linked to this, it is worth noting that unexpressed anger is hypothesised by Malan to be the precipitating problem in very many cases (1979, p95-6, table 2) which, in contrast to the case of the pesticide chemist, do not involve difficulties in which the theme of "precariously keeping something at bay" is observed at all. In some of these cases Malan is admirably straightforward in saying that "nobody knows" why a certain stress sometimes gives rise to a given symptom. For example, in the case of the "Drama Student" (1979, p33-4) Malan hypothesises that unexpressed anger (and jealousy) have given rise to a fear of travelling underground, a link which Malan declares to be mysterious. But is it not plain that this link could be easily "explained" in symbolic terms - why not, for example, suppose that

this patient's fear of travelling underground to be her way of "symbolically avoiding" emotions she has repressed, that is, emotions she has "forced underground", as one might metaphorically express it? It is indeed the very ease, thus illustrated, with which such suggestions can be made which is the problem for this approach.

Secondly, Malan argues for a "symbolic connection" between the pesticide chemist's stress and his obsessional difficulties without paying due attention to alternative explanations. Malan argues that the "pesticide chemist" is angry because he is trying to please everyone by performing tasks to perfection (and attempting to have them so performed by his subordinates). But is it not, therefore, far more straightforward to suppose that the "pesticide chemist's" precarious efforts to keep at bay task demands are the source, rather than the symbol, of this patient's anger? On this view, it is because of perceived unreasonable demands at home and work that the pesticide chemist is both anxious and angry - anxious that he may not be able to meet all of these demands and angry that they have been placed upon him. And indeed, is this not precisely what Malan himself claims when he argues that this patient's anger results from his concern and efforts to please everyone? Malan's intervention, it might similarly be argued, is helpful because it enables the patient to exercise, through his greater assertiveness, effective control over these perceived task demands at home and work which are a genuine source of stress for him. It appears, therefore, that there is no necessity to hypothesise any symbolic connection to make sense either of the difficulties of the "pesticide chemist" or his progress in therapy.

3.6.4.4 The patient's purposes

A third feature which might be argued to give Malan's discussion a psychodynamic character is the manner in which it aims to make sense of the pesticide chemist's difficulties in terms of his purposes. Malan sees the patient as trying both to avoid and control his own anger. In order to achieve these desired ends, Malan suggests, the patient is both not acknowledging his anger, and subjecting it to symbolic control. Both the pesticide chemist's lack of awareness concerning his emotional state, and his obsessional difficulties, are thus seen as things which the patient has done or produced, not things which have happened to him. In the discussion that follows attention will focus on the claim that the patient has produced his lack of awareness concerning his own emotional state.

Let it be allowed, then, that the "pesticide chemist" is, as Malan suggests, out of touch with his own anger when he presents for treatment. Malan appears just to assume, given this, that the patient has produced this state of affairs - or at least, Malan suggests that some such account as this follows from our not merely dismissing the patient's problems "as something

mysterious for which there is no point in looking for an explanation" and instead "take them seriously and try to explain them in terms of intelligible human anxieties..." (Malan 1979, p102). Yet given the complexity of some emotional reactions one clearly cannot just assume that this is so - why should it not be simple error that causes a person such as the "pesticide chemist" not to understand the emotions he is experiencing ? (Klein's account of "splitting" in neonates has been criticised on broadly similar grounds by Ryle, 1990.)

Why should Malan assume the contrary? Maybe one influence on this, discussed by Ryle (1949), is the tendency to think that people have a "privileged access" to their own emotions and motives - much as they might be argued to have such an access to, for example, their own aches and pains. A closely allied tendency is to think that people also have incorrigible knowledge of their own emotions and motives. The possession of such knowledge would imply that whenever somebody's honest self-report demonstrated an ignorance of his own emotions or motives this could only be because of self-deception on the part of that person - because, that is, the person has failed to face something which he knows to be true.

Against such a position, it need only be remarked that Ryle (1949) correctly points out that we have no incorrigible knowledge of our own emotions and motives, and such knowledge cannot, therefore, provide grounds for what is evidently Malan's implicit assumption that people must always have some reason for not being fully aware of their own emotions and motives.

3.6.4.5 Insight

A fourth feature which arguably helps to make Malan's work recognisably psychoanalytic is his treatment recommendations. As discussed above, he sees insight as being the motor of change in therapy, and it will be recalled that Malan believes the best symptomatic improvement to occur as a result of the patient coming to understand the nature of the hidden feelings he putatively has towards his parents. The belief in the potency of this kind of insight is at least arguably peculiar to psychoanalytic writers, albeit not shared by all of them (see, for example, Symington's remarks quoted earlier).

What evidence is there for insight being important in the case of the "pesticide chemist"? This question will be considered first as regards the "triangle of conflict" and then as regards the "triangle of person".

(a) The triangle of conflict

According to Malan's account, the pesticide chemist became, during his therapy, (1) more aware of his anger concerning a number of things in his life, (2) more able to give appropriate expression to this anger, and (3) less handicapped by his obsessional difficulties.

Why does Malan take, consistent with his account of "Koch's postulates" (section 3.6.2.2), "(1)" to be the fundamental change (Malan 1979, p108) - especially given the fact that, as noted earlier, elsewhere in his discussion (p107) he states that OCD patients may gain insight of this kind without any therapeutic effects? Besides, if it is, as Malan himself argues (1979, p96-7), "constructive assertion" which is supposed to be therapeutic for this patient, it is difficult to see why his merely becoming aware that he needs to be more constructively assertive should in itself be of help. Similarly, why should the patient's merely being made aware of anger which he is supposed to be anxious about feeling make him less anxious about, and thus more able to express, that anger?

Given these points, then, would it not be more plausible for Malan to suggest that "(2)" is the most important factor producing "(3)"? The most crucial change for this patient, on this view, is not his becoming more aware of his anger, but his becoming more successful in dealing with it - his becoming more appropriately assertive - with his awareness of his own anger perhaps only increasing as a result of his becoming more assertive (this may indeed be what happened to two of Emmelkamp and van der Heyden's [1980] patients who achieved insight into their own lack of assertiveness only after they had begun assertiveness training - see Chapter 7).

(b) The triangle of person

Malan does not offer any argument in favour of insight into the triangle of person being important to the "pesticide chemist". Thus, despite the favourable therapeutic outcome reported, we are told that the "pesticide chemist" himself (Malan 1979, p105) has made nothing of the suggestion that his childhood relationship with his father was the origin of his difficulties in resisting the demands of others, and Malan reports that the patient says "he was not aware of such feelings [i.e.unexpressed anger] in his childhood". Furthermore, even if the patient had experienced an improvement in his obsessional difficulties in response to such an interpretation, it is surely clear that this cannot in itself be strong support for the truth of this interpretation, contrary to one of Malan's "four categories" of evidence (see section 3.6.2.3). Similarly, if this interpretation had "deepened the rapport" between the "pesticide chemist" and his therapist, it is clear that this too would not be in itself confirmation of the truth of this interpretation, contrary to another of Malan's "four categories". If our concern is with the truth rather than the therapeutic value of the interpretation concerned, the effects of this interpretation on treatment outcome and/or therapeutic rapport are clearly only acceptable tests of the interpretation on the assumption that it could not effect the patient in these ways if it were false. And why should one accept this assumption? Is it not, indeed, clear that people

can be deeply effected by all manner of false beliefs, and in some cases favourably effected, in the sense of being able to be more happy about their lives?

Taking the triangle of person and conflict for the pesticide chemist together, therefore, one may in conclusion state that there is probably no good reason for supposing that insight into either of these hypothesised triangles is of central importance for this patient, contrary to Malan's account of how psychodynamic therapies work in general, and of how the "pesticide chemist's" therapy worked in particular.

It may be further pointed out that other arguments Malan uses provide no good evidence in favour of the patient's childhood relationship with his father being at the root of his difficulties. This claim will be examined in the section which follows.

3.6.5 The role of childhood experience

Some of the issues discussed by Malan as regards "the triangle of conflict" for the "pesticide chemist" raise testable questions. As is argued elsewhere (Chapter 7), for example, the hypothesis that a patient's being insufficiently assertive is making a contribution to his difficulties lends itself readily to empirical investigation. It is relatively easy to attempt to manipulate the patient's level of assertiveness, and the hypothesis that this is an important factor in the production of his distress has clear implications as to the effects that this manipulation, if successful, should have.

The situation regarding the issues raised by the "triangle of person" for the "pesticide chemist" is quite different. It is obviously not possible to manipulate the variables involved here, for example, the past behaviour of the patient's father, the patient's childhood reaction to this behaviour etc. in order to see what effect such manipulations would have upon the patient's present difficulties. How, then, does Malan attempt to support his suggestions as to the "triangle of person" for this patient?

What Malan would perhaps regard as the major justification of these suggestions is that the relationship between the "pesticide chemist" and his father should strike us as providing a plausible "common sense" explanation of the patient's problems in adult life. The appeal is to our "common sense" understanding that "the child is father to the man" and how such childhood experiences will therefore make certain problems in adult life more likely to occur.

How powerful is this appeal in the present case? The heart of Malan's argument seems to be that the patient's relationship with his father is supposed to bear similarities to his relationship with his wife and boss. Suppose it be granted for the sake of argument (and contrary to the pesticide chemist's own remarks regarding his relationship with his father) that

these similarities do obtain - that all these relationships involved the pesticide chemist in not expressing his angry feelings at what he perceived as the unreasonable treatment he received. Can these similarities not be explained in terms of the pesticide chemist's childhood difficulties with his father simply being another instance of exactly the same character disposition observed in this patient as an adult which produced his difficulties at home and work - his being poor, that is, at expressing his feelings and demanding what he wants? Why suppose this disposition of character to have originated in his relationship with his father instead of merely having been instantiated there, its cause being more a matter of, for example, innate factors? Malan readily suggests that his patient's disposition of character is at the heart of his adult problems yet appears entirely to overlook the contribution this factor may have made to his childhood experiences. If the adult is held to be able to create relationships of the kind in question largely or entirely by his own behaviour, why should this capacity be largely or completely denied as regards the patient as a child?

Perhaps in reply to this and in support of the primary importance of childhood relationships it might be argued that adults choose as their spouses and friends etc. people who resemble their parents. It is, then, on this view, being able to make such choices which (i) is a major reason why adults are able to exercise more control than children over the nature of the important relationships in their lives and (ii) explains how - in influencing or determining these adult choices - childhood relationships are of primary importance. Perhaps, then, applying this approach to the "pesticide chemist", he might be argued to have picked both a spouse and boss with whom it is not possible to be angry despite their unreasonable behaviour? On this argument, his spouse and boss were people with whom others would also have found it difficult to become angry. It is thus not this difficulty but rather the "pesticide chemist's" choice of these people as his spouse and boss which reflects the effect of his childhood relationship with his father on his adult life, and thus establishes the primary importance of that relationship.

But this cannot be a complete answer to the suggestion that it may be simply the "pesticide chemist's" disposition of character which produced the difficulties in both his childhood relationship with his father and his adult relationships with his spouse and boss. This is because the theme of anger not being expressed is also supposed to appear in the patient's relationship with his therapist, according to Malan's "triangle of person", and the selection of the therapist is a decision over which the patient will presumably have exercised little or no choice. If Malan's "triangle of person" is accepted, therefore, it seems that the theme of anger not being expressed in the "pesticide chemist's" various relationships is

unlikely only to be a matter of the people with whom he chooses to interact being of a certain type. (While a full discussion of the point is beyond the scope of the present account, it is worth just noting here that some psychodynamic formulations seem as if they, by contrast, probably overstate the contribution a person made as a child to the nature of his or her relationships with adults - that is, these formulations make precisely the opposite error to that which it has been argued Malan may make as regards the "pesticide chemist".)

Perhaps there are other possibilities which might suggest the "pesticide chemist's" relationship with his father to be of primary importance? For example, the good intentions with which his father was absent from home - attempting to earn money for his family - might have made it genuinely inappropriate to be angry with him. Despite this, it might be suggested, his son was angry with him and it is this which produced the "pesticide chemist's" difficulties in expressing anger even when this is appropriate in adult life (Malan himself appears to express this view at one or two points in his discussion). According to this position, then, it is not merely the similarities between the "pesticide chemist's" childhood relationship with his father on the one hand and his adult relationships on the other which justify Malan's assigning the former its primary importance. Alternatively or additionally, it might have been that the "pesticide chemist" was actively encouraged as a child not to express his feelings.

But the evidence in favour of these possibilities seems to be at best weak. Thus, there is no suggestion at all in Malan's discussion of the patient having been encouraged as a child not to express his feelings. Similarly, it appears as if the behaviour of the "pesticide chemist's" father was not beyond reproach and that the patient may, indeed, have been quite badly neglected by his father. We are told, for example, that the patient "had hardly any home life as a child" and, with his father "constantly away" he "involved himself in activities away from home seven nights a week where in fact he found a substitute father who took him under his wing" (Malan 1979, p105).

3.6.6 The elimination of body products

There is a further theme which is arguably of importance in characterising Malan's account of the pesticide chemist's difficulties as psychodynamic, although this theme is not very prominent in Malan's discussion of this case. This theme is that of the elimination of bodily products. In discussing the case of the "pesticide chemist", Malan, formulating the problem as having to do with unexpressed anger, lays stress upon the fact that when this patient did finally break down "his outburst resulted in his making a mess" (1979, p103, original emphasis); a similar point was also offered to the patient by his therapist who suggested that "the feelings that he was so terrified of were strongly aggressive feelings, and

that they were to do with the part of him that wanted to let go all his controls and make a mess" (1979, p104).

These points are referring us back to a discussion earlier in Malan's chapter. There, Malan suggests that (p99) "it is an empirical fact that in some people [the elimination of bodily products] take on a significance apparently out of all proportion to their simple function of expelling unwanted matter from the body".

Malan suggests that there are at least "two important roots" to this exaggerated importance. The first of these is "some kind of primitive equation between the inside of the body and feelings, so that elimination of physical matter becomes equated with the expression of feeling"(Malan 1979, p99, original emphasis). The second is that the "struggle of toilet training may come to represent the whole issue of freedom versus restraint; and incontinence which results in making a mess may become a way of expressing anger and rebellion" (Malan 1979, p99, original emphases). It is further suggested by Malan that a later struggle over the right to express sexual feelings may "reawaken the earlier struggle, and the two may become associated " (p99). Although he does not say so at this point, Malan would wish to suggest that this association is facilitated by sexual activity also involving (at least for men) the elimination of body products - see for example Malan's discussion (p100) of the difficulties of the "Maintenance Man" with nocturnal emissions.

These various suggestions would appear directly to owe more to Freud's work than do many of Malan's remarks, and are worth dwelling upon a moment. It is clear from his case discussions that while Malan does present the theme of the elimination of body products as linked to OCD, he does not regard this link as peculiar to the disorder. For example, the case of the "Maintenance Man" mentioned above, which involves a phobic problem, features this theme far more prominently than does that of the Pesticide Chemist. Malan can thus only be arguing that the elimination of body products takes on a great significance for some people, a proportion of whom happen to be OCD patients.

Malan's position is surely controversial in suggesting that the exaggerated significance of body products has to do specifically with the expression of anger (see below). What cannot be denied, however, is that there are some OCD patients for whom contamination with such body products as urine and faeces does have an exaggerated significance. What, then, of Malan's suggestions as to why this should be the case? How well does he explain the disgust or fear of these expelled body products?

Malan's two reasons for this need to be considered separately. But before doing this, let two points be noted. Firstly, those OCD patients who are strongly inclined to avoid contact

with urine and faeces etc. are distinguished from the normal population in degree rather than kind in being so inclined - these products have, in Malan's terms, an "exaggerated significance" for most people. Secondly, some measure of disgust also seems to be felt by most people towards contact with many other materials which are expelled from inside the body. Thus, this attitude is not restricted to those materials which would have been involved in a person's toilet training or "the later struggle over the right to express sexual feelings". For example, contact with phlegm, saliva, sweat, vomit, food or drink which has been spat out and perhaps even ear wax are all usually in some measure aversive for most people (tears would seem to be an exception), with this aversion being particularly marked when the saliva, sweat, etc. is that of another person (the contact between lovers, and between young children and their carers, provide some exceptions to this rule).

To return, then, to Malan's two reasons for the exaggerated importance of body products, the second of the points introduced above looks as if it may be inconsistent with the second of Malan's reasons. Why should one, for example, suppose toilet training to be of fundamental importance to our understanding of the "exaggerated significance" of urine and faeces when similar attitudes are found towards other materials which are expelled from the body which have never been involved in such training?

A more parsimonious explanation would clearly apply to the aversion which is felt towards most body products, not just some of them, and Malan's first reason for this aversion - that there is a "primitive equation" between the inside of the body (and materials which have come from there) and "feelings" - appears, in contrast to his second reason, to possess this wider applicability. This explanation, however, meets with other difficulties. The suggested link between the inside of the body and feelings is implausible as an explanation of the aversiveness of body products - it seems that to equate matter from inside the body with feelings would not render such matter aversive in any reliable way, because it is obviously not the case that most or all feelings are aversive. A different explanation is called for, and might perhaps be supplied, contrary to Malan's position, in terms of an innate aversion to body products as possible sources of contamination (see, for example, Gray 1982, discussed in sections 3.3.4 and 3.3.5 above).

3.6.7 Summary

Four features have been argued to give Malan's discussion of the case of the "pesticide chemist" its psychodynamic character - a character it possesses despite Malan's not making use of psychoanalytic notions concerning psychosexual developmental stages and infancy experiences etc. These four features were the account's reference to "unconscious" mental

processes, its formulation of the patient's concerns as having symbolic significance, its claim that the patient's difficulties may be understood as having occurred in order to serve some purpose for him, and its claim that insight into hidden feelings derived from childhood experience is of fundamental importance to therapeutic outcome. It was argued that if there is anything of a psychodynamic character about the first of these features, this is merely a semantic matter. The other three features are all more substantive, but reasons have been offered for thinking none of them to be important to the understanding of the pesticide chemist's problems. Objections have also been brought against a number of points Malan makes as regards "body products", although none of these points is of central importance to his discussion of the case of the "pesticide chemist". One may thus conclude that insofar as Malan's account is psychodynamic in character doubts are justified as to its value. One cannot in turn conclude from this, of course, that psychodynamic approaches have nothing to offer in explaining any case of OCD. Nonetheless, it is hoped that this discussion has raised a number of difficulties which it seems likely psychodynamic approaches to OCD and indeed other disorders will often encounter.

It seems appropriate at this point to note that the foregoing discussion also shows the psychodynamic aspects of Malan's account to be open to objective criticism, contrary to Popper's (1972) characterisation of psychoanalysis. This point is, of course, double-edged from the point of view of Malan's discussion - in defending the various psychodynamic aspects of Malan's account from the charge of not being refutable it also suggests these various aspects of his account to have been refuted (or at least to have been shown to be implausible).

Is there anything, then, in Malan's account which could by contrast be argued to illuminate the difficulties of the "pesticide chemist" and other OCD patients? Some of Malan's remarks made in connection with the "triangle of conflict" for the "pesticide chemist" seem as if they may do so, although it is being suggested here (albeit perhaps somewhat stipulatively) that these remarks do not contribute to the specifically psychodynamic character of Malan's account. Malan's suggestions that the patient's unassertive behaviour contributed to the production of the patient's problems and that "constructive assertion" has helped to relieve these problems are in particular noteworthy and have, indeed, received further attention from Emmelkamp and his co-workers who, working from a behavioural perspective, have used "assertiveness training" with both OCD patients and agoraphobics. Their work is discussed, along with some further comments on the theme of unexpressed anger and OCD, elsewhere (see section 3.4.7.3 and Chapter 7).

3.7 Chapter three conclusions

It is evidently the difficulties rather than the successes with which all of the foregoing theoretical approaches to OCD meet which is most striking, at least if one attempts to regard any of these approaches as providing a full explanation of the disorder. Nonetheless, some of the accounts which have been considered certainly offer observations concerning some OCD patients which may well be of importance to any account of their psychopathology which eventually emerges. Thus, the possible role of evolutionary influences in selecting the content of some OCD symptoms (stressed by some behavioural writers and some of the Pavlovian personality theorists), the neurotic temperament of many OCD patients (stressed by the Pavlovian personality theorists), the co-morbidity of OCD with certain other disorders (stressed by both the Pavlovian personality theorists and Janet's account) and the tendency to timidity and unassertiveness which was noted in at least some cases of OCD (stressed by Janet's account as well as the psychodynamic case discussion offered by Malan) have all been presented as examples of such observations. It has also been noted, furthermore, that the last of these observations may also be used to make suggestions concerning the treatment of at least some cases of OCD (also see Chapter 7).

The use of exposure with response prevention as a treatment for OCD is at least arguably the most important therapeutic advance associated with any of the theoretical approaches discussed above, the use of this intervention being "associated with" the behavioural approach in the sense that it has been most enthusiastically championed by behavioural writers (notwithstanding Pitman's remarks regarding Janet's treatment recommendations). It has been pointed out that debate as to the mechanisms by which this intervention may work continues (also see Chapter 7).

The observations presented above as being of possible theoretical importance could neither in themselves nor in conjunction with one another support a full account of OCD. Thus, a full account could not be supported by the evolutionary considerations discussed above because these considerations cannot by themselves plausibly explain clinical, as opposed to normal, levels of fear or discomfort concerning such matters as dirt and contamination, and cannot explain the contents of many other kinds of OCD difficulty at all. Tendencies towards timidity and neuroticism could similarly not support a full account of OCD - both of these characteristics appear in conjunction with disorders other than OCD and are in any case evidently insufficient to account for such features as the bizarre thinking, repetitive behaviour and failures of memory concerning everyday actions which are reported by OCD patients. These features can similarly not be very readily explained in terms of evolutionary arguments.

The account of OCD considered in the next chapter offers yet another approach to these features, claiming that they reflect the cognitive characteristics of OCD patients, characteristics which are, this account suggests, primary to the disorder.

Chapter four: Cognitive style/deficit approaches to OCD

4.1 Reed's account

4.1.1 Reed's hypothesis of the central problem

A number of authors (for example, Beech and Perigault 1974, Beech and Liddell 1974, Carr 1974, Volans 1974, 1976, Reed 1985,1991) have suggested that a generalised cognitive style or deficit in OCD sufferers may help explain the occurrence of their symptoms. Reed's ideas represent the most fully worked out contribution from this school. He advances what he terms a "cognitive-structural approach" (Reed 1985) to explain both the obsessional personality (and personality disorder) and OCD. Following Reed, the term "obsessional" will be used generically in what follows to cover OCD, the obsessional personality/personality disorder, and the person who exhibits either of these.

What, then, is the central problem in obsessional disorders, according to Reed? His attempts to answer this question in just a few words tend to be a little cryptic. For example, he suggests that obsessional traits and symptoms reflect difficulties in "spontaneous categorizing and integration" (1985, p220) leading to attempts to compensate for these difficulties by "the artificial over-structuring of input, of fields of awareness, of tasks and situations" (1985, p220); there is, he argues, a "maladaptive over-defining of categories and boundaries" (1969[ii], p787). Reed states that the central phenomenon of these disorders "may thus be seen as a striving towards boundary fixing or the setting of limits in the cognitive/perceptual modalities" (1985, p220).

What all this amounts to may perhaps be made a little clearer by reflecting upon what Reed presents as one example of an experimental confirmation of it. Reed (1969[ii]) presented subjects with blocks of various shapes, sizes and colours and asked them to form classes of these blocks in any way they thought sensible, that is, to choose for themselves the features on the basis of which the blocks were to be classified.

Reed correctly predicted that his obsessional group (all of whom were diagnosed as suffering from obsessional personality disorder) would tend to assign fewer blocks to each of the classes they formed than would controls, and would therefore require more classes to complete this task. Reed suggested that this was because his obsessional subjects, in contrast to his controls, would tend to experience doubts as to the appropriate ordering of the categorisable features of the blocks in terms of their task relevance and importance. In contrast to controls, that is, Reed suggested that his obsessional subjects found it more difficult to order these features "hierarchically", tending to experience doubts as to whether remote or unlikely differences between blocks were as important as obvious similarities. This rendered

it more difficult for them to be certain that any two (or more) blocks were sufficiently similar to be classified together.

Consider an example. Suppose that among the blocks presented to subjects were two red cubes of slightly different sizes and a blue pyramid. Reed's suggestion is that obsessional subjects would be more likely than controls to feel uncertain that the similarities between the cubes in terms of their shape and colour were more important than their slight difference in size for the purposes of classification on this task. This renders it both more difficult for these subjects to make a spontaneous decision as to how to classify these blocks, and less likely that they will classify them in the most straightforward way - in this case, in terms of colour and shape, thus placing the red cubes together and separate from the blue pyramid. It is also more likely that they will need more categories than controls to classify the blocks. Thus, Reed suggests that having failed, in contrast to controls, spontaneously to discard "torturous and arbitrary" complicating features (minor differences in size in the present case) as being of little importance to the task, these complicating features are also more likely to turn up among the criteria chosen by obsessionals to classify the blocks. Thus, the obsessionals would, relative to controls, be more likely in the example above to classify the two cubes separately, as well as keeping them both separate from the blue pyramid.

Returning, then, to the terms in which Reed advances his hypothesis, the difficulties of obsessionals in "spontaneous categorizing and integration" are, in the case of the block sorting task, their uncertainties as to the ordering of the categorisable features of the blocks in terms of their relevance and importance. Their "artificial overstructuring" of the task, their "maladaptive over-defining of categories and boundaries", is their tendency to include among the criteria employed in forming their classes "remote and unlikely" features of the blocks, and thus to form a larger number of classes. This inclusion of such features is also an example of the obsessionals' "striving towards boundary fixing or the setting of limits".

Reed's use of this task was to examine what his obsessional subjects would do in what he describes as an "inductive situation". The task requirement, that is, is "to induce concepts or classes given an array of class members (rather than deduce which members belong to a given class)" (Reed 1969[ii] ,p787). Reed (1969[i]) also reports a confirmation of his hypothesis with a "deductive task", but his position is that obsessionals will face "most difficulty in open-ended tasks requiring an intuitive approach and/or <u>inductive</u> reasoning" (Reed 1985 p194, original emphasis). He has presented an experimental confirmation of this suggestion, using once again patients diagnosed as suffering from obsessional personality disorder (Reed 1977[ii]). It seems, then, that it is the performance of obsessionals on Reed's

inductive classificatory task to which we should turn for what he would present as the best analogue of the difficulties experienced by obsessional patients.

Consider, then, Reed's approach as applied, for example, to compulsive hand-washing. The non-obsessional subject experiences no difficulty in feeling satisfied that his hands have been "properly" or "sufficiently" washed; in particular, minor details regarding the exact manner in which his hand-washing has been performed, or the precise feel of his hands after washing them etc., do not determine whether or not this satisfaction is achieved. This is in contrast, Reed would suggest, to the compulsive hand-washer (1985, p165). Doubts regarding such details as these will prevent him feeling satisfied that the task has been properly done. This, then, is the failure of the compulsive handwasher to "structure or integrate" this task spontaneously. Ritualistic hand-washing, in which a detailed specification is made as to exactly how this task is to be carried out, is the attempt of the hand-washer to compensate for this difficulty in spontaneously structuring or integrating the task by artificially over-structuring it.

Reed claims that these difficulties with spontaneous structuring and artificial over-structuring are the "central psychological phenomenon" (1985 p117) of all the "classical symptoms" of OCD - although remarks he makes elsewhere in his discussion concerning "redintegration" suggest that he would only wish to defend a somewhat qualified version of this thesis (see section 4.1.2).

Reed also presents (1985, ch 5) a discussion of thirty-three traits which are most commonly accepted as being among the constituents of the obsessional personality and personality disorder, and argues that these too are susceptible to analysis in terms of his hypothesis.

Reed's position might appear from the foregoing to be tautological. What we mean - or at least one of the things we can mean - by saying that someone is obsessional as regards his performance of any task is that, as compared with others, he pays undue attention to the details of that task. Reed's hypothesis is in essence this claim that details are noticed more by obsessionals, and the hypothesis therefore fails, it might be argued, to make any empirical claim at all.

This objection to Reed's position can be rejected. Reed is explicit as regards two points to which his analysis gives rise, both of which are substantial, and indeed highly controversial, empirical claims. Firstly, Reed suggests that the thinking style detailed in his hypothesis is the <u>primary dysfunction</u> in obsessional disorders. To return to the hand-washing example discussed above to illustrate this, Reed's position is that the attention to detail

described is not the result of any emotional problem, such as, for example, a greater than normal fear of germs or disgust at dirt. The distress reported, he suggests, is rather an effect of the sufferer's way of performing the task - an effect of its being very difficult for him to feel satisfied that the task has been adequately performed. Reed thus suggests that the distress which these disorders involve stems not from that which the sufferer thinks about, but rather from his manner of thinking about it; not, similarly, from that which the sufferer does, but rather from his manner of doing it (Jakes 1987). Putting this rather differently, Reed argues that the problems "of obsessionals concern the form [of experience] rather than [its] content" (1991, p88, emphasis added) (see section 3.5.3.1 for an interpretation of some of Pitman's [1987{ii}] remarks which is similar to this view).

Reed believes, consistent with this account, that the major mood disturbance in obsessional disorders is not anxiety, and he argues that both behavioural and psychoanalytic theorists place too much emphasis upon this emotion in their explanations of obsessional disorders. He (1985, p137) suggests that "there appears to be no convincing evidence that anxiety plays a significant role in obsessional disorder" and argues that, where anxiety does occur, "it is usually related to long term apprehension, rather than the immediate experience", that is, the hand-washer's anxiety will concern such matters as the possibility of his never getting over his problem, rather than dirt or germs, according to Reed. Reed lays stress upon there being other kinds of mood disturbance which are frequently reported by obsessionals, such as depression, anger and frustration, these being distress of a type which, he suggests, may be readily explained, again consistent with his hypothesis, as the result of the sufferer having difficulties in spontaneously structuring the tasks which provoke these emotions.

Reed stresses (1968, p388-9) that patients often report their decision difficulties not "to bear any direct relation to the importance or emotionally-chargedness" of the situation in which they arise, such difficulties often occurring as regards details the patient himself recognises to be "prosaic, trivial and unthreatening" (Reed 1991, p80). Reed provides examples of patients whose greatest agonies of doubt concern whether or not to tie their shoelaces or whether or not to get up out of a chair. Another example would be that of having to place a book on a table so that its edges are parallel to those of the table, despite no outcome of any significance being feared should this not be done (see section 1.3.4.6). Reed argues that such phenomena support his account of obsessional disorders, and suggests that "content approaches [he includes behavioural and psychoanalytic approaches, as well as most other kinds, in this category] are less likely to be productive and generalisable than approaches that aim to uncover the form or structure of obsessional thinking in general" (Reed 1991, p80-1).

The second controversial empirical claim which is central to Reed's position is his suggestion that the thinking style detailed in his hypothesis is general to the sufferer's functioning - is part, Reed suggests, of the sufferer's personality structure - rather than being specific to particular symptom areas. A close relationship between the obsessional personality (and personality disorder) and OCD is therefore proposed, the symptoms of the latter being, according to Reed's account, "simply pathological extensions" (Reed 1985, p117) of the traits of the obsessional personality and personality disorder. While Reed can, therefore, allow that symptoms other than those which are classifiable as OCD may be experienced by someone with an obsessional personality, he is committed to the view that such symptoms cannot emerge in the absence of this personality (or even, perhaps, in the absence of the personality disorder).

4.1.2 Reed on therapeutic approaches

In the light of his analysis, Reed suggests that it is the central task of interventions with obsessional disorders to change the patient's style of cognitive functioning. Although Reed suggests a number of treatment techniques (1985, p225-6) designed to achieve this, he unfortunately presents no data as to how effective these techniques are.

In addition to these techniques, Reed also notes "some popular therapeutic measures which, in the light of the present theory, are either contra-indicated or at least time-wasting" (Reed 1985, p221). Of most interest, he includes among such therapeutic measures assertiveness training and behaviour therapy, including exposure with response prevention. Reed suggests that assertiveness training does not seem to be "even remotely relevant to obsessional disorder" (1985, p222), such patients tending to be opinionated and over-assertive (this supposed feature of people with obsessional disorders presumably follows, according to Reed, from their over-structuring approach).

Regarding exposure with response prevention, Reed argues that the achievements of this approach have been confined to the reduction of compulsive behaviour, with little or no progress having been made as to the remediation of obsessions (1985, p208); Reed appears to hold that this supposed outcome is pretty much what his account could have predicted for behavioural approaches.

Reed also objects to the use of cognitive therapy with obsessionals, although here it is what Reed takes to be the defining criteria of the symptoms of OCD which are the basis of his objection. He points out that Beck's therapeutic techniques, for example, involve encouraging the patient who is suffering from depression or anxiety to gain insight into the irrationality of the thinking which is postulated, by this therapeutic approach, to be the primary

difficulty in these mood disturbances. This approach is inappropriate for obsessional disorders, Reed (1985, p213) argues, because by definition these patients already have insight into the thoughts which preoccupy them.

4.1.3 The "redintegation" hypothesis

Reed (1977[i], 1985) supplements his account with the suggestion that some obsessional symptoms reflect the "faulty redintegration" of memories, this supposedly resulting from the patient's hypothesised problems in structuring experience.

Why does Reed make this suggestion, and what does it amount to? Reed (1977[i], p178) correctly points out that his original hypothesis "makes no direct presumption about mnemonic processes". Thus, his original hypothesis attempts to explain why a patient may feel unsure that, for example, some act has been properly performed, or some thought adequately considered, but makes no attempt to explain why a patient should not feel sure that he has or has not even attempted to perform some act. The obsessional patient, that is, is hypothesised by Reed to have difficulties in spontaneously organising the details of his performance, not in recollecting these details themselves.

Yet some symptoms do appear, Reed points out, to involve difficulties of recollection which are of this kind. Reed himself, indeed, suggests that "perhaps the most central feature of obsessional disorders seems to involve pathologically faulty memory" (Reed 1977[i], p177, original emphasis), and he suggests (1977[i], p177) that the "doubts and indecision...manifested in compulsive checking...are often of the "Did I or Didn't I?" variety". These patients, that is, sometimes do doubt whether they have carried out certain actions at all, rather than doubting only whether these actions have been performed properly, this point being especially clear in those cases where the patient fears that he may have performed some horrific act, such as, for example, strangling an elderly next door neighbour. The doubt here could evidently not be "Have I strangled this elderly person properly?"; it would, rather, be "Have I done this terrible thing at all?".

Reed thus raises a difficulty for his account in its original form which he attempts to solve by introducing the notion of the "faulty redintegration of memories". Memories which exhibit this feature have an "attenuation of the personalised element", Reed explains (1985, p154) - the person recalls what he has done or what has happened to him, but in a manner such that it is as if he has not done these things or had these things happen to him.

Reed suggests that the memory difficulties of obsessional patients are of this kind. Contrary to what might seem to be the case, Reed tells us, an obsessional does not, for example, check his door because he doubts he has locked it. Reed cites the fact that such

patients often report that they know their door is locked before checking it in defence of this claim. Thus, in explaining the checker's need to repeat his action, Reed says "...something paradoxical about compulsive checking is indicated. For the compulsive checker often reports that he knows that the door is locked before he checks....the question seems to have already been answered.....[yet] the compulsive checker's doubts have not been allayed; his question, then, has not been answered. But, as his check has, in fact, answered the question of whether he had locked the back door, his continuing dubiety strongly suggests that that was not his question" (Reed 1985, p151, original emphases).

What is unsatisfactory about the recollections of these patients which causes them to check is not, according to Reed, "the factual content of their remembering, but the quality of the remembering itself" (Reed 1985, p154, original emphasis). Repetitive checking, Reed suggests, thus "represents an attempt to invoke a satisfactory level of redintegration" (Reed 1977, p154). He attempts to support this contention with the following extracts from the accounts of his patients: "It's done, I know that - but I can't see myself doing it.....", "I think I remember all right. But it's blurry somehow - as though I'm not there.....", "I know they are O.K. When I think back I can see them in my head. The trouble is that I can't be sure that it was me seeing them before....." (Reed 1985, p153, original emphases).

Attempting to link this point to his central hypothesis, Reed suggests, following Bartlett (1932), that this "personalized flavour of reminiscence must reflect...the level and scope of schematization of the original experience" (Reed 1985, p197). "If the anankast suffers impairment in the organization and integration of experience, this would imply some attenuation in schematization (including the personal element)", Reed argues (1977[i], p178).

Objections to this analysis will be considered later. Attempting for the moment to be clear as to exactly what is being argued, consider Reed's suggestion a little more closely. Reed appears to be arguing that the repeated actions which result from putative difficulties in redintegration do not involve the patient in adjusting minor details of each successive action, and similarly do not involve the patient's worrying that the details of the performance of these actions have been inadequate, in contrast to the hand-washing example discussed above (see section 4.1.1). The patient's "doubt" simply concerns the lack of a "subjective flavour" to his recall, according to Reed, and the act is repeated in an attempt to establish such a flavour. (The patient is evidently also supposed by Reed to be unaware that the absence of this subjective flavour is in fact caused by his poor spontaneous structuring, otherwise the patient presumably would attempt to adjust minor details of his actions in an attempt to improve the "structuring", and thus, on Reed's view, his redintegration of, them.)

All of the examples Reed discusses in terms of redintegration are to do with checking and rumination. Furthermore, the discussion Reed offers of a case of hand-washing (1985, p166) seems very clearly to be an account of this phenomena in terms of his original hypothesis unsupplemented by the notion of redintegration. Are these examples, then, considered by Reed to be typical? Is Reed's redintegration explanation being applied to checking and rumination, and not to cleaning? The present writer has been unable to find a discussion of these points in Reed's work, but it does seem as if Reed's theoretical position would make this difficult to sustain. The problems in redintegration, after all, are supposed to result from those in spontaneously categorising, so he would presumably expect most or all obsessionals to exhibit both and to tend to do so in the same situations and on the same tasks. But being clear as to Reed's position regarding his redintegration hypothesis is in any case rendered rather difficult by his seemingly failing to incorporate this hypothesis into summaries of his position. Thus, he suggests (1985, p221), in stating the essence of his theory, that checking and ruminations, the two phenomena he had previously discussed (1985,p197-198) in terms of poor redintegration, may be seen as "reflecting a failure in "terminating response", coupled with uncertainty in categorical limit attribution ("Is it finished?"). This applies to inner reasoning ("Has everything been considered?"), to mnemonic brooding ("Are my recollections sufficient and correct?"), and to the consideration of activities ("Did I do it properly?"). The questions are insoluble, because such qualifiers as "finished", "sufficiently", "satisfactorily" and "properly", cannot be defined in any ultimate and fixed manner" (Reed 1985, p221). The notion of redintegration would appear to have been entirely overlooked here.

4.2 Objections to the "redintegration" hypothesis

There are a number of objections which apply specifically to this redintegration supplement, as opposed to Reed's account as a whole. Firstly, the quotes from patients by which Reed tries to support his "reintegration" hypothesis feature such comments as "...but I can't see myself doing it", "...it's as though I'm not there" etc., made as these people describe their attempts to recall previous checks etc. they have carried out. Yet it is surely at least unclear that such locutions as these amount to more than mere expressions of doubt as to whether or not these checks have been made, contrary to Reed's hypothesis. There is, furthermore, no claim on Reed's part that these choices of words are systematically used by obsessionals. These extracts are merely examples which have been quoted precisely because of the particular choice of words involved. It seems likely that checkers often express their doubts in quite other ways, and so too much stress is probably being placed by Reed on the

particular terms used by the patients he quotes.

Secondly, Reed uses (1985, p151) the fact that the obsessional will often say that he knows, for example, that his door is locked before checking it to support his claim that the checking does not concern the state of the door at all, and rather concerns the quality of the patient's memories. But there may be difficulties with this argument, because Reed is prepared to recognise that in cases of obsessional difficulties not involving redintegration at all, the patient will still report insight of this kind; indeed, he argues elsewhere (Reed 1985, p4-5) that this feature must, by definition, be present in every case of OCD (see section 1.2.2), whereas he evidently believes that the redintegration hypothesis can be applied to only some kinds of obsessional problem.

Thirdly, Reed takes as his starting point in introducing the notion of reintegration, the "did I or didn't I" doubts of obsessional checkers and ruminators. But can this account explain why doubts should be reported at all by these patients? On this hypothesis the checker has no doubts as to what has occurred, he simply has a memory which lacks a given property - its "subjective flavour" - and the patient's repeated actions are supposed to be an attempt to add this property. So where does the patient come by his doubt, according to Reed?

One reply which might be made here is that the lack of a "subjective flavour" to the patient's recall in some way leads him to doubt whether or not the remembered event or action has taken place. But Reed at least could clearly only make this reply by sacrificing the claim, emphasised in presenting his "redintegration" supplement, that the patient's doubts and repeated actions are not to do with checking the state of the door at all.

Fourthly, Reed's redintegration hypothesis fails to explain all of the phenomena with which it was introduced to deal. This included not only cases where patients repeat actions which they have already performed, but also cases where they fear that they may have performed some action which has in fact not been performed, such as some violent behaviour. How can such cases be explained in terms of the level of redintegration of the original experience when there has been no original experience? If Reed's redintegration account can be extended to explain these cases, he at least does not suggest how this should be done.

4.3 A motivational problem for Reed's account

As seen, Reed's account attempts to explain why an obsessional patient might find it difficult to feel certain that, for example, his hands have been properly cleaned. Yet if the patient were entirely indifferent as to whether or not his hands were so cleaned, there would be no problem for him, however difficult it was for him to feel certain that this state had been achieved.

What thus appears to be taken for granted by Reed's account is that such a patient needs to feel certain that his hands have been properly washed. This raises an important problem for Reed's account, because the patient's behaviour suggests that his problem is not just a matter of his having more difficulty feeling certain that actions have been properly carried out. A greater intolerance of such feelings of uncertainty is additionally or instead implied. The compulsive handwasher, for example, appears to be less able than others to bear the possibility that his hands may be contaminated; it matters more to him that they are not. And Reed's account appears to be silent as to why this should be so. Thus, while most non-compulsive washers would no doubt prefer their hands to be properly clean - at least at mealtimes and the like - they will, in the ordinary course of things, be able to tolerate the thought that they are not. If, for example, time is short, or the non-compulsive hand-washer finds himself in a frame of mind such that he simply cannot be bothered to visit the bathroom, he will be able to bear not merely the uncertainty that his hands may not be properly washed, but indeed the certainty that they are not. We may conclude that the inability to determine whether or not tasks such as hand-washing have been properly carried out cannot be the only factor which distinguishes obsessional patients from others. This difficulty will be referred to as the "motivational problem" in what follows (the same difficulty was introduced for part of Pitman's [1987{ii}] account in section 3.5.3.1).

The same problem may be introduced as regards those cases in which obsessional patients are distressed by matters which they themselves recognise to be trivial and prosaic, cited by Reed as especially favouring his hypothesis. Consider the example presented earlier of having to place a book on a table so that it is perfectly parallel to the table edges, despite no outcome of any significance being feared by the patient should he fail to do this. While the patient may in this example describe the situation of the book not being parallel with the table edges as trivial, he is nonetheless unable to tolerate the book being placed in any other manner. This, then, surely distinguishes him from a non-obsessional person - such a person will simply not care whether or not the book has been so placed. The obsessional does not, therefore, differ from others only (if indeed at all) in terms of how precisely he judges whether or not the two edges are parallel. The same problem for Reed's account therefore arises - a greater intolerance of the book's not being parallel to the table edges is implied, and his account provides no explanation of this.

(The same objection may also be raised against Reed's redintegration account. Even if it were true that, for example, a checker's recall of his action lacks a "subjective flavour", why should this matter to the patient? Why, on Reed's account, is this quality so important

to the checker that it is worth his repeating his actions many times in an attempt to establish it? Reed's redintegration hypothesis evidently just assumes that it is, from the patient's point of view, worth his repeating his action in order to establish this.)

The following two replies to these points, which might be suggested in defence of Reed's account, would seem to be unsuccessful. Firstly, not all non-obsessional patients will be able to tolerate the thought that, for example, their hands have not been properly washed. Obsessionality is a continuum and some proportion of the population who will never become obsessional patients are, it might be plausibly argued, nonetheless a sufficient distance along this continuum not to be able to tolerate this thought. What separates the population of obsessional patients from these normals could therefore be, on the present argument, an inability to determine that, for example, their hands have been properly washed and it is this which Reed's account could be used to explain. Cleanliness, on this argument, matters equally to both groups, and it is only the greater inability of the obsessional patients, as explained by Reed's hypothesis, to determine whether or not things are clean which renders their difficulties more severe.

But this reply only shifts the problem elsewhere, rather than solving it. The inability spontaneously to determine that one's hands are properly clean is being acknowledged by this attempted defence of Reed to be insufficient to distinguish the sub-clinical group of obsessional normals (along with obsessional patients) from the remainder of the normal population. Consistent with the "motivational problem" for Reed's account, it is conceded by this defence of Reed that cleanliness matters more to the sub-clinical group (and obsessional patients). If Reed's account is to attempt to explain obsessionality per se, rather than merely attempting to explain obsessionality of clinical severity, therefore, the motivational problem still stands. His account still says nothing as to the distinction which is being argued by this defence to obtain between this subclinical group and the remainder of the normal population. Besides, it seems unparsimonious to explain the difference between a sub-clinical group with an above average degree of obsessionality and the remainder of the normal population entirely in terms of one factor, and the difference between this sub-clinical group and the clinical obsessional population entirely in terms of another.

A second possible reply which a defender of Reed's position might use is based upon an independent claim of Reed's (1968, p390), that compulsive behaviour increases discomfort and mood disturbance (see section 4.10 for a discussion of this claim). This suggestion has also been put forward by other authors, for example Walker (1973), Beech and Perigault (1974), Beech and Liddell (1974).

This claim might be used to argue that the obsessional patient's greater intolerance of the thought that, for example, his hands may not have been properly washed is in some way secondary to the increasing distress which is being experienced as his compulsive behaviour proceeds. He begins his hand-washing, for example, according to this reply, with no greater need to feel certain that his hands have been properly washed than the normal subject does. His inability to establish that his wash has been adequate, however, as explained by Reed's account, leads to frustration and other mood disturbance which, in turn, leads (perhaps, for example, by increasing the subject's sensitivity to punishment) to an increase in the importance to the patient of his hands being properly cleaned. It is this, this reply would conclude, which leaves the obsessional unable simply to leave his hands feeling that they have not been properly washed, in contrast to what many normal subjects are able to do.

There are at least two counter replies here. Firstly, some authorities (for example, Rachman and Hodgson 1980) would argue many compulsive rituals to be discomfort reducing, contrary to Reed (see section 4.10). Secondly, this defence of Reed implies that if the obsessional were not even to attempt his hand-washing ritual, this would create for him no difficulties greater than those encountered by a non-obsessional subject who decided to omit a hand wash. This is because the obsessional's not even attempting to wash his hands would bypass, according to this defence of Reed, the increase of mood disturbance which is produced by the obsessional's inability to establish that his hands have been properly cleaned. Against this implication, however, it appears that for many OCD patients not performing their compulsions provokes a very high degree of discomfort (de Silva 1988, p205), suggesting that this defence must be incorrect, for at least the majority of OCD patients.

A more promising approach might be to postulate a mood disturbance which is not an effect of the postulated thinking style of obsessional patients. This mood disturbance, it could be suggested, operates in conjunction with that thinking style to produce the distress of these patients. Accounts of this type have been put forward by, for example, Beech and Perigault (1974), Beech and Liddell (1974) and Claridge (1985) (see section 3.3.7 for a discussion of the latter account). Two points should be noted here. Firstly, these approaches amount to an abandonment of Reed's central claim that the thinking style of the obsessional patient forms the single root of his problems. Secondly, the possibility of incorporating Reed's account into an approach like this demonstrates that the foregoing "motivational problem" cannot in itself argue Reed's account to be unworthy of further research. It may still be able to provide a partial account of obsessional disorders, even if the foregoing arguments show that it cannot provide a complete account of them.

4.4 A revision of Reed's account

How might Reed's account be revised to meet the "motivational problem" introduced above? One possible adjustment is the suggestion that it is not in establishing whether or not tasks have been properly completed, that the patient's thinking style is crucial but rather in his attempts to establish what are likely to be the outcomes of his not properly completing these tasks.

As pointed out above, Reed suggested that the obsessionals in his block sorting task tended to experience doubts as to whether remote or unlikely differences between the blocks were as important as obvious similarities. He also argues that this reflects a general approach to things by the patient in the "cognitive/perceptual modalities" (1985, p220). Could he not argue, then, that this style of functioning will also pervade the manner in which the patient calculates the probable outcomes of the tasks which cause him difficulty, that is with doubts being experienced as to whether remote and unlikely possible consequences are as probable as outcomes which are in fact much more likely? To give two examples of this hypothesised effect, (i) this revised version of Reed's thesis would suggest that the problems of the obsessional patient with cleaning difficulties result from his having doubts about, or overestimating the likelihood of, serious illness being contracted from normal household dirt and dust, and (ii) this account would similarly suggest that the problems of the obsessional patient with checking difficulties result from his having doubts about, or overestimating the likelihood of, fires or other household disasters, being caused by normal household switches and plugs which have not been turned off or removed.

These adjustments to Reed's account would appear to answer the "motivational problem" which confronts Reed's thesis. Thus, tasks such as checking and cleaning matter more to obsessionals, according to this revised version of Reed's thesis, because these patients are more inclined to think dire consequences likely to follow from such tasks not being properly completed.

It must be stressed that Reed himself does not appear to put forward anything like this revised version of his account, perhaps only because he believes his account to be a sufficient explanation, perhaps because he would actually object to this revised version as failing to make good sense of the clinical phenomena.

It should also be noted that this revised version of Reed's thesis suggests obsessionals will be more likely than others to be objectively mistaken in the calculation of probabilities. This is in contrast to Reed's block sorting task discussed in section 4.1.1, where there were only different styles of sorting the blocks, no right or wrong way of doing so, and

where no objective mistakes as to the calculation of probabilities were involved. (It should also be noted that the block sorting task discussed in section 4.1.1 therefore stands as a weak analogue, and consequently also a weak confirmation, of this revised version of Reed's thesis.) In what follows, the revision of Reed's account achieved by the introduction of the hypothesised difficulties of obsessionals in handling probabilistic information will be referred to as the "revised thesis".

A rather different attempt to revise Reed's thesis might be mentioned in passing. This attempt would suggest that rather than overestimating the likelihood of aversive outcomes, obsessionals are instead more inclined simply to care or worry about such outcomes. Thus, on this account, the obsessional does not overestimate the probability of, for example, his house flooding from a dripping tap, but he does worry more about such things happening. This greater degree of worry is quite consistent with his regarding this outcome as extremely unlikely, this approach would argue.

While this may indeed be a consistent hypothesis, it cannot, thus stated, be plausibly represented as an adapted version of Reed's thesis. This is because no reference is made to the patient's thinking style in explaining why he worries more than others about the possibilities which he recognises as much as they do to be highly remote. It might be replied to this that the "thinking style" involved is precisely the hypothesised tendency to worry more - to have more "anxious cognitions", perhaps - as to these remote possibilities. But this, then, is to be told that the patient worries more about remote possibilities because his thinking style is such as to worry more about remote possibilities. This tautology obviously contains nothing which suggests that this position has anything at all to do with the account presented by Reed.

There are some important differences between the "revised thesis" and Reed's original account. For example, the "revised thesis" cannot be very readily applied to the "trivial and prosaic" situations and tasks which provoke compulsive behaviour and which are so emphasised by Reed. At most, it seems that all this position could suggest is that in such cases there must be some highly unlikely feared consequence of not, to return to the earlier example, placing a book on a table in a certain way, the probability of which the obsessional has overestimated. That the patients to whom Reed is referring are unable to report such feared consequences would have to be put down to a lack of insight on their part, on this adjusted account, rather than taken at face value, as Reed himself takes these reports (by the same token, Reed's account is less readily applicable than the "revised thesis" to those cases where fears of highly unlikely outcomes are reported).

Similarly, distress will no longer be entirely secondary to doubts and compulsive

behaviour, according to the "revised thesis" - this thesis argues that the discomfort experienced by obsessionals will include the fear which, it suggests, results from the patient's thinking that unfavourable outcomes are likely to occur. Doubts and compulsive behaviour will be considered to be at least in part the result of such discomfort, reflecting the patient's concern that the feared outcome not occur (see below for some objections to the account of such doubts and behaviour the "revised thesis" provides). Perhaps, then, these implications of the revised thesis would lead Reed to reject it as inconsistent with what he believes the clinical phenomena to be?

The "revised thesis" may seem very like the position of a theorist such as Carr (1974) who suggests that appraisals of threat are heightened in OCD patients. These patients make, Carr argues, an abnormally high estimate of unfavourable outcomes occurring in any situation. The "revised thesis" is not, however, equivalent to such a position as this. In a situation of objectively high risk, it seems that the "revised thesis" would make precisely the opposite prediction to that made by an "enhanced threat appraisal" explanation, that is, predicting that the obsessional should be inclined, in this situation, to experience doubts as to whether the remote or unlikely chance of a favourable outcome is as probable as the unfavourable outcome which is in fact more likely. The account of some cases of OCD offered by Volans (1974, 1976) may have more in common with the "revised thesis" (her work is discussed more fully in Chapter 6).

4.5 Objections to the "revised thesis"

While addressing the "motivational" problem for Reed's account, the "revised thesis" faces difficulties of its own. Why, it might be asked, according to the revised thesis, does the obsessional have problems in convincing himself that his hands are properly clean, or all his light switches off, leading to repetitive cleaning or checking, accompanied by doubts about his compulsive behaviour, and low levels of discomfort reduction following the carrying out of this behaviour? Can this only be a matter of the perceived likely consequences of failing to carry these tasks out successfully? Would such an account not more readily predict that a very careful performance of the act of cleaning or checking should be enough - just as one no doubt observes cautious, but not endlessly repetitive, behaviour from normals in situations of objective high risk, for example a surgeon washing his hands before performing an operation (also see section 3.2.5)?

To this point it might be replied that the endless repetition of action by obsessionals, when observed, reflects the nature of the tasks with which the patient has difficulties. Thus, according to this reply, the obsessional is struggling to complete tasks which in objective

terms it is very difficult to be sure one has adequately performed. It is this, in conjunction with the obsessional's thinking unfavourable outcomes to be very likely if such tasks are not adequately performed, which produces the patient's profile of repeated, but unsuccessful, attempts to convince himself that these tasks have been so performed, this reply suggests.

This view, however, meets with problems very like those encountered by Rachman and Hodgson's (1980) account of the contrasts between checkers and cleaners (see section 3.2.5). When the tasks the obsessional struggles to complete are considered in objective terms, difficulties in being sure as to their successful completion would appear to apply more to the typical cleaning task than to the typical checking task. Thus, it would seem, from the objective point of view, comparatively easy to be sure that, for example, all of the plugs in a room have been removed from their sockets - one only has to look in order to see that this is so. This is less true of the task of ensuring that, for example, one's hands are perfectly free from all germs and other invisible contamination. One would, then, if anything expect, on the basis of these "nature of task demands" points, that it would be cleaners rather than checkers who would tend to show more repetitiveness in their compulsions, more doubts as to how effective their compulsive behaviours have been, and less relief from discomfort from their compulsions. In fact, as noted in section 3.2.5, the situation is precisely the reverse.

It is, then, not possible to explain the low levels of relief, repetitiveness and doubting exhibited by many of the obsessional patients who report checking difficulties in terms of a combination of the "revised thesis" and the nature of checking tasks. This in turn argues that it is unparsimonious to attempt an explanation of the rarer cases of cleaners whose compulsions exhibit these features in terms of a combination of the "revised thesis" and their tasks demands, more plausible though this argument might have seemed when considered in isolation from the foregoing discussion of checkers. Besides, cleaning tasks carrying objectively high risks appear not to produce endlessly repetitive behaviour and doubting in normals, as this explanation of the behaviour of obsessional cleaners would appear to predict (see section 3.2.5). One may conclude that the revised version of Reed's thesis, while answering the "motivational" problem outlined above, is silent as to the repetitiveness and doubting present in some compulsive behaviour.

The situation as regards the "revised thesis", in other words, is almost precisely the reverse of that found with Reed's account. There, the patients difficulties in feeling certain that (some) tasks have been properly completed were addressed, but the obsessional's stronger motivation properly to complete such tasks were not. With the revised thesis, the matter is the other way about. While an attempt is made to give an account of the stronger motivation

of obsessionals to complete tasks properly, what is not explained is their doubts about these tasks having been properly performed, and their repeated attempts so to perform these tasks.

What then, it might be asked, of simply combining these positions, Reed's account being used to explain the obsessional's doubts that tasks have not been properly performed (and the obsessional's resulting repetitive behaviour), the "revised thesis" then being added to this to explain why it is so important to the obsessional that tasks have been so performed?

The main objection to this combined approach may already be clear from the foregoing. Reed himself suggested (see section 4.1.3) that his original account does not explain all compulsive behaviour, and introduced difficulties for this account specifically (or so it seemed) as regards the doubting and repetitive behaviour of checkers (and ruminators). These difficulties Reed introduced were argued earlier to be sound (section 4.2). The doubting and repetitive behaviour of checkers (and the doubting of ruminators) cannot be explained, therefore, simply by combining Reed's original account with the "revised thesis" - both of these approaches encounter difficulties in explaining checking behaviour.

There is a further difficulty for the "revised thesis". A tendency to overestimate the probability of unlikely aversive outcomes occurring can scarcely be specific to obsessional disorders. There seem, for example, to be no theoretical grounds for supposing that many phobic patients will be distinguishable from obsessionals in these terms. No doubt many obsessional patients do fear outcomes that strike us as unlikely, and indeed in some cases it seems to be the very remoteness of the feared possibility which helps to give the patient's anxiety an obsessional, as opposed to a phobic, quality (see section 1.3.4.4). But many phobic subjects would nonetheless appear to fear things as unlikely or even more unlikely than those feared by many other obsessionals. Many if not all deluded subjects, to take another example, seem similarly inclined to think some events or situations probable or certain to occur (or to have occurred), despite these events or situations being in fact at best highly unlikely (see section 1.3.9 and 1.3.10). Many delusions, phobias and obsessional difficulties (no doubt among other psychiatric disorders) will, therefore, be indistinguishable from the point of view of the "revised thesis", which will consequently be silent as to why one patient develops one of these disorders while another develops one of the others. This point is especially worth noting in connection with the experimental investigations conducted by Volans (1974, 1976), Huq et al (1988) and Garety et al (1991), as well as that reported in Chapter 6. These investigations have examined the handling of probabilistic information by obsessional and deluded patients, and some of these investigations (see Chapter 6) have reported contrasting results from these two groups; the "revised thesis" would appear unable to make much sense

of this finding. (Reed's account, by contrast, does not appear to encounter any difficulties in distinguishing obsessional disorders from conditions such as delusions and phobic states - his account emphasises features such as the doubting and repetitive behaviour of obsessionals, which distinguish their difficulties from these other conditions; Reed's account, indeed, appears if anything to overstate the differences between phobic and obsessional disorders - see section 4.12 below.)

What, then, remains of the "revised thesis" after considering all of the foregoing arguments? These arguments leave open the possibility that miscalculations of probabilities may help explain various aspects of obsessional disorders. For example, both the fears and the compulsive behaviour of a typical cleaner in Rachman and Hodgson's (1980) investigation might be open to such an explanation (as noted in section 3.2.5, Rachman and Hodgson report that the compulsive behaviour of such patients tends not to exhibit the repetitiveness and high levels of doubt which the "revised thesis" has difficulty explaining). Fears of remote or unlikely consequences are also reported by many of these patients with cleaning difficulties, similarly consistent with the "revised thesis".

The fears reported by many checkers also involve a concern with highly remote consequences. Even if the "revised thesis" cannot explain the doubting and repetitiveness which is frequently observed as regards the compulsive behaviour of such patients, therefore, it may still be consistent with the improbable outcomes which concern many of them, and thus be able to provide a partial explanation of their difficulties (note that it was in effect argued above [section 4.2] that Reed's account would be unable even to provide this). One may conclude from these points that the "revised thesis" may be worthy of further research even when the foregoing criticisms of it have been taken into account, although the thesis is unable in its present form to explain the differences between obsessional difficulties on the one hand and phobias and delusions on the other.

A number of further important theoretical difficulties confront both Reed's account and the "revised thesis"; these will be reviewed in the next two sections.

4.6 Further objections to Reed's account

4.6.1 Obsessional problems not involving doubts,indecision or ritualising

There are many obsessional difficulties where the characteristics stressed by Reed's account - doubting, indecision and ritualising etc. - do not occur at all, or are not the only or primary feature observed. Consider the following three examples: (i) discrete obsessional thoughts - instead of a series of doubts or a train of argument, some obsessional patients may be troubled by single thoughts such as "Christ was bastard", "I want to sleep with my mother",

"I wish my husband were dead". Sometimes discrete visual images are reported as the source of distress, these typically involving such contents as "mutilated corpses, decomposing fetuses, my husband involved in a serious motor accident, my parents being violently assaulted" (Rachman and Hodgson, 1980, p11); (ii) obsessional impulses - some patients experience the impulse or urge to do something they regard as unacceptable, for example to perform some harmful or violent act or to say something of an offensive nature (also see section 1.3.4.2); (iii) superstitious thinking - such thinking is a prominent feature in some obsessional problems, the patient thinking it necessary, for example, to touch or arrange household objects in a certain way in order to prevent such events as car accidents or other misfortunes which are in reality entirely unrelated to the behaviour concerned (also see section 1.3.4.4).

Reed's account seems to be unable to explain these symptoms, and the comments he offers as regards both obsessional impulses and discrete obsessional thoughts appear to confirm that this is so. He suggests that obsessional impulses "are often difficult to classify being not so much urges to action as fears, doubts and misgivings about urges" (Reed 1985, p17, original emphasis). Surely very few workers in this field would agree with Reed on this point, recognising that the impulse or urge may occur in the absence of such doubts and misgivings, and may itself often be the source of distress for the patient? As regards discrete obsessional thoughts, Reed tells us that these are less common than "older textbooks suggest" (1985, p17) and reports as a confirmation of this (1985, p18) that only two of the ninety-seven obsessions observed in a sample of fifty of his patients were of this form. But Reed's classificatory scheme also includes numerous other categories, for example, an "obsessional fears" category, which accounts for 31% of the obsessions in his sample, and it is not possible to tell from Reed's discussion how many of the obsessions in these other categories might be reclassified as "discrete obsessional thoughts".

Reed's account also appears to be unable to explain why superstitious thinking should occur. Such thinking is, in particular, clearly not a matter of the patient's merely "overstructuring" a task, although the patterning of behaviour and objects etc. may be observed. The patient is not simply taking greater pains than others to attend to the details of his performance of some ordinary task as he would in a case of "overstructuring". He is, rather, attempting to carry out some entirely abnormal "task". The very performance of his action, not merely the manner of this performance, stands in need of explanation.

The "revised thesis" is in a somewhat similar, but not identical, position as regards these three phenomena. It too can offer no account of obsessional impulses and is similarly silent as regards at least most cases of discrete obsessional thoughts too. It is, however, able

to make better sense of symptoms involving superstitious thinking than is Reed's original account. The "revised thesis" was, after all, primarily introduced to explain obsessional symptoms where the feared outcome reported was of a highly unlikely nature. It seems that this thesis could deal with superstitious thinking as an instance, or extension, of such fears.

4.6.2 Obsessional problems which do involve doubts and indecision

Problems also confront Reed's account as regards many cases where the doubts and indecision stressed by this account are observed. Firstly, as has been pointed out elsewhere in criticism of "cognitive style/deficit" accounts in general (for example, Rachman and Hodgson 1980, Emmelkamp 1982), the difficulties of obsessional patients often involve doubts which concern only a few tasks or situations, or even only one task or situation, with which they have to deal. Why are multiple doubts concerning an endless variety of different tasks and situations not observed, as would be predicted by Reed's account?

Reed's reply here is straightforward; it is, he says, a "significant fact that severe obsessionals seldom suffer from a single, discrete obsession" (Reed 1985, p24). It is "puzzling", according to Reed (1985, p25) that Rachman and Hodgson (1980) report 73% of their patients to have a single, predominating obsession. Reed similarly suggests that Rachman and Hodgson misrepresent Akhar et al's (1975) findings. Whereas, according to Rachman and Hodgson, Akhar et al reported 75% of their patients to suffer from a single, predominating obsession Reed states that 51% of the patients in this study had multiple obsessions (Reed 1985, p25).

But even if correct, this still leaves 49% of Akhar et al's sample without multiple obsessions, as well as Rachman and Hodgson's findings to be explained. Reed hints as to how he might tackle this point: "obsessional people are often embarrassed about their experiences, and when forced to seek help they typically report only the most incapacitating of their problems" (Reed 1985, p24). Clearly, there may well be something in this point, but it risks degenerating into the unfalsifiable claim that any patient who reports only a single obsession would tell us of further obsessions if only he were less embarrassed, more forthcoming etc. In the absence, therefore, of positive evidence that more of the patients of the kind questioned by Rachman and Hodgson (1980) and Akhtar et al (1975) would have reported multiple obsessions in other circumstances, we must conclude that many obsessionals probably do experience only single or just a few obsessions, and that Reed's account makes poor sense of this observation.

A somewhat related point is that the content of many obsessions appears to be highly selective, with certain themes - contamination, sex, violence etc., - featuring far more

frequently than would be expected by chance alone (Akhar et al 1975) (also see sections 3.2.2, 3.3.4, 3.3.5 and 3.4.5.5).

How might Reed, then, explain why obsessions should tend to concern some themes much more often than others and/or why many patients do not suffer from multiple obsessions? As mentioned earlier (section 4.1.1), he does suggest that most difficulty will be encountered by obsessionals when performing tasks requiring "an intuitive approach and/or inductive reasoning" (Reed 1985, p194), but it seems clear that this suggestion is not sufficiently specific either to explain the highly selective nature of the content of many obsessions or those patients by whom only one or just a few obsessions are reported.

In his very last contribution to the topic of obsessional disorders, Reed makes a further suggestion - that "the personal significance of the item under consideration...will load the cognitive functioning affectively, and may thus accentuate its operating characteristics" (Reed 1991, p79, emphasis not in original). In the same chapter he similarly suggests, commenting on Person and Foa's (1984) findings (see section 5.2), that "obsessional decision making may be made even more difficult when the material to be sorted is of a disturbing nature. The question invites empirical examination" (Reed 1991, p83). Indeed, in the conclusion of the same discussion Reed even argues that "whether this [cognitive] style operates in all mental activities or whether it emerges only in the context of threatening events is as yet unresolved" (1991, p96). This last remark, however, is out of step with all of Reed's other statements on this topic, even in this last discussion - for example, both those quoted above and his further comment (1991, p79-80) that "...by definition, the cognitive style is employed in processing the material, the material does not engender the style...contents are limitless, but the form is invariant". Besides, to concede that the patient's dysfunctional cognitive style might only arise when he is dealing with disturbing material is to take up a position such as Claridge's (1985) (see section 3.3.7.2) which, as noted earlier (section 4.3), abandons rather than qualifies Reed's central claim that the thinking style of obsessional patients is the heart of their distress. And so long as this claim is insisted upon (even if one does allow that contents may exercise some influence over form) it seems that one cannot very readily explain either the selectivity of the contents of obsessions, or the patients who suffer from just one or a few obsessions. Reed's account, therefore, so far as the present point is concerned, is more plausible as regards those patients who suffer from an obsessional personality disorder or from multiple obsessions with numerous idiosyncratic contents. It otherwise needs to be at least supplemented if it is to accommodate the selectivity of the contents of obsessions and the fact that many patients appear only to suffer from one or a few obsessions.

How, then, might Reed's account be so supplemented? One possibility is as follows. Rachman and de Silva's (1978) findings indicate that "normal obsessions", which have very similar contents to the abnormal obsessions suffered by patients, are common in the experience of many people. This, it might be suggested, could be used to help explain why the abnormal obsessions which are experienced by patients tend to have the contents they do. These contents, according to this argument, are to be explained in the same terms as those in which the contents of normal obsessions are explained, these terms being assumed to be independent of Reed's account. The postulated thinking style of obsessional patients could then, on this account, be introduced to explain why the patient is so incapacitated when he tries to settle such doubts as, for example, whether or not he has harmed somebody while walking down the street. It might be similarly postulated that the mechanisms responsible for the production of obsessions with these contents also determine the number of obsessions by which the person is troubled, and thus help explain those cases where only one or just a few obsessions are experienced.

There are problems for this supplemented version of Reed's account. Firstly, an explanation will still need to be provided as to why the thinking style of these patients only operates on thoughts etc. with such contents, and not (or at least not to the same extent) on other thoughts etc. Unless some account of this can be provided, the problems of the selectivity of content and single, predominating obsessions, re-emerge. Secondly, it would appear that the doubting experienced by patients who suffer such obsessions as "Have I harmed someone as I walked down the street?" is often not of the kind required by Reed's account, that is, involving a string of various "what if?" questions in which numerous remotely possible ways in which this could have occurred are considered. (That many such doubts appear not to take this form is what led Reed himself to introduce the "redintegration hypothesis" - see section 4.1.3.) Thirdly, this supplemented version of Reed's thesis lacks parsimony. Whatever the manner in which this account would attempt to explain the "normal obsessions" which are experienced by many people, it is essential to the account that this cannot be in terms of the "cognitive style" postulated by Reed, which the account does use to explain the difference between normal and abnormal obsessions. A more parsimonious account, then, would not seek to explain normal obsessions in one way, and the difference between these and abnormal obsessions in quite another. It will be recalled that a similar difficulty was raised for another supplemented version of Reed's account, discussed above (see section 4.3) (the same difficulty has also been introduced in connection with Salkovskis's account of OCD [Jakes 1989{ii}]). The supplemented version of Reed's thesis under

consideration here fails, then, like that discussed in section 4.3 to explain obsessionality per se, attempting instead merely to explain only obsessionality of clinical severity.

4.7 Comments on Reed's remarks regarding therapeutic approaches

Numerous points need to be made regarding Reed's comments on the remediation of obsessional disorders (see section 4.1.2). Firstly, Reed claims that the achievements of behaviour therapy have been confined to the reduction of compulsive behaviours, with little or no progress having been made as to the obsessional experiences which provoke such behaviour. In Reed's view, the patient's style of cognitive functioning would have to be changed in order to reduce or eliminate the patient's obsessional experiences and Reed believes, as discussed earlier, that this can be achieved by running patients through "cognitive exercises", including tasks in which the "stress is placed upon speed of performance rather than accuracy" (Reed 1985, p225), the crucial ingredient to the intervention, in Reed's view. Yet in exposure with response prevention, it is precisely this kind of approach which is often adopted, only with respect to the activities which provoke the patient's major difficulties, rather than on artificially constructed tasks (which can surely only make the intervention more powerful, if it makes any difference at all, on Reed's view). A patient, for example, may be asked while on a programme of exposure with response prevention to complete his cleaning and checking tasks much more quickly than he has done hitherto, that is, with the stress being placed upon the speed of performance, not its accuracy. Why, then, given Reed's own therapeutic recommendations, should he believe that this will have no impact on the patient's "obsessional experience" - his fears, doubts, urges to clean or check etc. - and will only alter the compulsive behaviours provoked by such experiences?

Reed may in any case misrepresent the outcome data from behaviour therapy with obsessionals. It is true that behaviour therapists have so far had limited success with obsessional difficulties involving only covert phenomena (Lovell et al 1991). But where covert phenomena, such as doubts, fears, urges etc., do provoke compulsions, it has been argued by some authors (for example, Rachman and Hodgson 1980) that exposure with response prevention has a considerable impact in many cases on both the patient's obsessions and compulsions, achieving, it is claimed, considerable success with both phenomena (although see section 3.3.6.2).

Secondly, Reed's suggestion that assertiveness training is misconceived with obsessionals because such patients are opinionated and over-assertive must be challenged. Janet (1903) for example, remarked upon the unassertive nature of many of his obsessional patients (see section 3.4.4). As noted earlier (section 3.4.5.2), Lewis (1936, p328), postulated

two types of obsessional personality, "the one obstinate, morose, irritable, the other vacillating, uncertain of himself, submissive". It seems clear that only the former group correspond to Reed's description, the latter presenting precisely the opposite profile. Ingram (1961 p1017-8, discussed by Rachman and Hodgson 1980), claimed that only just under half of his obsessional patients could be described in one or other of these ways, but he found twice as many of the submissive type of patient as of the obstinate/morose type, among those who could be so classified. Indeed, elsewhere in his discussion Reed himself (1985, p200) notes (as a paradox) that "[obsessionals] are...inflexible and un-shifting [yet] they are also reported to be unassertive and vacillating". There is also some empirical evidence against Reed's claim that assertiveness training cannot help obsessionals (Emmelkamp and van der Heyden 1980). This evidence, along with some remarks as to its implications for our understanding of obsessional disorders, is discussed elsewhere (see section 3.4.7.3 and Chapter 7).

Thirdly, contrary to Reed's objections to the use of Beckian cognitive therapy (for example, Beck 1976) with obsessional patients, not all of these patients do regard their obsessions and compulsions as absurd (see sections 1.2.6.2, 1.2.6.3, 1.3.7 and Chapter 7); indeed, Reed himself (1985, p5) acknowledges that patients report differing degrees of perceived senselessness with respect to them.

4.8 Further comments on Reed's account, and the supporting evidence cited by Reed

4.8.1 The nature of Reed's causal claim

It was earlier pointed out that Reed stresses two points in presenting his thesis - firstly, that the thinking style detailed in his hypothesis is the primary dysfunction in obsessional disorders, that is, the cause of the distress experienced by obsessional patients, and secondly, that this thinking style is general to the sufferer's functioning. Note that while the first of these statements makes a causal claim, the second does not. The first claim is, furthermore, logically independent of his second. There is no contradiction in the suggestion that a patient's thinking style has caused his distress in some particular area of his functioning while also supposing that style to be specific to that area of the patient's functioning (such an account would, of course, beg the question as to why the patient's thinking style is different in that one area, hence the objections to Reed's account considered in section 4.6.2 above).

4.8.2 An explanation of the hypothesised thinking style of obsessionals?

Does Reed also have an explanation for why obsessionals exhibit the thinking style he attributes to them? In an early paper, he introduces this issue but leaves it open: "given that obsessional thinking does, in fact, reflect a failure in the spontaneous structuring of experience, this failure itself has still to be explained. It is possible to formulate explanatory hypotheses

in terms of both psychoanalysis and learning theory. On the other hand, the present hypothesis [i.e. his account] has the merit of encouraging the search for explanations from other viewpoints" (Reed 1968, p391). (These possible behavioural and psychoanalytic accounts Reed mentions would have, of course, to be quite unlike existing behavioural and psychoanalytic accounts in continuing to explain, consistent with Reed's account, the distress of obsessionals as resulting from their cognitive style.)

None of these possible explanations of the hypothesised cognitive style of obsessionals has been followed up in Reed's subsequent work, and his last full discussion (1985) of both psychoanalytic and behavioural theories contains no suggestion that these approaches might be able to provide such an explanation. This is, of course, a perfectly legitimate position for Reed - having suggested that the distress of obsessionals is an effect of their hypothesised cognitive style he is not required also to provide an explanation for this cognitive style.

4.8.3 Reed's experiments

As discussed above, Reed attempts to support his account with a number of experimental studies (1969[i], 1969[ii], 1977[ii]) and this work has been followed up by a number of other authors (see Part C below for a discussion of several of these studies). These investigations attempt to demonstrate that the thinking style Reed hypothesises to be basic to obsessional disorders may be observed in the performance of obsessional patients on various "neutral" tasks, involving such matters as block sorting, verbal tests and arithmetical problems etc. ("neutral" here means unrelated to the contents of the patient's obsessional difficulties). What would be the significance of obsessionals performing on these tasks in the manner Reed predicts, and in particular, could we infer from it that the thinking style in question causes the distress of obsessional patients, as Reed's hypothesis suggests?

The intended argument from these experimental tasks to this causal claim may be illustrated by the following example. That an obsessional patient washes his hands paying close attention to the details of his performance clearly does not, in itself, confirm Reed's account - this style of performance might be the result, as various different types of theorists would indeed argue, of the patient's fearing contact with dirt or germs etc. A neutral task such as block sorting, the argument continues, does not contain this ambiguity. If the same style of performance can be shown there as is observed on hand-washing tasks and the like, then it becomes unparsimonious, according to this argument, to explain that style of performance in the case of handwashing in terms specific to this task, such as the patient's having an abnormal fear of contamination etc. The argument suggests that it is most parsimonious to take this style, consistent with Reed's account, as the primary dysfunction, with the observed

discomfort as regards hand-washing being treated as its effect (the "trivial and prosaic" contents of some obsessional difficulties, discussed above (section 4.1.1), were supposed by Reed to argue the same point). This argument, then, attempts to establish one of the two claims Reed stresses in presenting his account - that the patient's cognitive style causes his distress - on the basis of the other of these claims - that the patient's cognitive style as observed in his problem areas may also be observed in his behaviour elsewhere.

This argument may be faulted in at least three separate ways (the premise of this argument - that the cognitive style of obsessional patients is general to their functioning and has been demonstrated to be so by experimental studies such as Reed's - is discussed elsewhere [see Part C]). Firstly, if the patient's difficulties are hypothesised to stem from some very general concern, such as a fear of being blamed or criticised for making mistakes, then the experimental task may be as relevant to this concern as the everyday tasks and situations which cause the patient's difficulties. According to this objection, that is, the patient may be concerned about being blamed or criticised for making mistakes on the experimental task too. This concern, the objection would continue, will produce the observed cognitive style on such tasks, just as it also produces it in the patient's problem areas. These points will be referred to as "objection [a]" in what follows.

Secondly, a fear, for example of dirt or germs, may often have required the patient over a long period of time to have made hair-splitting decisions as to whether or not things have been adequately cleaned. This style of dealing with possible sources of contamination could, this objection continues, have generalised from those tasks and situations in which dirt or germs are thought by the patient to be involved to other tasks and situations (including the experimental task), where even the patient would agree dirt and germs are not involved. This style may, that is, have becomes a habit for the patient, no longer tied to serving the ends it was initially introduced to serve. These points will be referred to as "objection [b]" in what follows.

(It is worth noting here that it is not the generalising of this style from one area of the patient's life to another <u>per se</u> which is crucial to this objection. It seems that Reed's account could allow for some such generalisation, and indeed, Reed's suggested therapeutic interventions evidently require such generalisations to be possible, albeit in reverse - a reduction in the patient's hypothesised cognitive style, that is, is supposed to arise first in the suggested therapeutic exercises and then to spread elsewhere in the patient's functioning. What is crucial to the present objection is that the cognitive style is hypothesised to have arisen in the first place only as a <u>secondary</u> phenomena, as a result of the patient's

hypothesised fear of contamination.)

Thirdly, patients may exhibit a particular thinking style both in experimental tasks and in problem areas such as hand-washing, due to a highly pervasive mechanism such as would be postulated by, for example, some psychodynamic formulations of obsessional difficulties. Thus, such a formulation might suggest that the patient's fear of being unable to control his own anger leads him to project that anger onto various situations and tasks and then to impose a rigid, detailed order upon - to "overstructure", in Reed's terms - these situations and tasks in an attempt to impose "symbolic control" over his anger (also see section 3.6.3.1, 3.6.3.2, 3.6.4.3 and 3.6.4.4). By assuming that these putative projective mechanisms are effecting a wide enough range of the patient's thinking and behaviour, this account would thus be able to make sense of the cognitive style featured in Reed's hypothesis being observed both in the patient's hand-washing and his performance on Reed's experimental tasks (as well as, of course, in a wide variety of the patient's other behaviour). This account implies, with the first objection above, that the patient will not find the experimental tasks a "neutral" activity at all, contrary to Reed's assumption. These points will be referred to as "objection [c]" in what follows.

To demonstrate, then, that obsessional patients have a distinctive cognitive style which may be observed both within his problem areas and on experimental tasks would not be to show that this style has a causal role in producing the patient's distress. What this observation would demonstrate is only that the patient is distinguishable from controls across a sufficiently wide range of his thinking and behaviour for this to be evident in the experimental tasks. And in the case of some obsessional patients, this demonstration would be neither trivial nor, indeed, easy to make sense of - this would be so in particular as regards those patients (see section 4.6.2 above) who report only a single obsession, whose difficulties seem relatively isolated within what is evidently an otherwise reasonably normal life, and those patients (see section 4.6.1 above) whose major difficulties appear not to involve the thinking style hypothesised by Reed at all. There are other obsessional patients, however, whose difficulties both involve their paying close attention to detail and appear to pervade many things, or even everything, they do. It would be unsurprising to find that the cognitive style of such patients was distinguishable from that of controls on experimental tasks. Patients with obsessional personality disorders would evidently all be of this latter kind, this point being especially worth noting as these are the patients with whom Reed conducted his experimental work.

The foregoing arguments against attempts to discover the primary dysfunction of

obsessionals by means of experimental tasks may be summarized as follows (these arguments may of course also be of relevance to the use of experimental tasks with other psychiatric groups). Suppose there are two features - "X" and "Y" - which have been observed together, or which it has at least been claimed occur together, in a given psychiatric symptom or trait of a personality disorder. In the case of obsessionals and Reed's theory, let "X" be the thinking style described by that theory, and "Y" the patient's emotional distress, be it anxiety, depression, anger or whatever. The use of experimental tasks is supposed to show that X is observable in the absence of Y rendering, it is claimed, the explanation of Y as being caused by X when they are observed together the most parsimonious account. The foregoing objections to this argument are, then, the claims that Y may be caused by the performance of these experimental tasks, this in turn causing X to appear there (objection [a]), that Y does not have to be caused by these experimental tasks to be the cause of X's appearance there (objection [b]) and that X and Y may be parallel co-effects of some third variable (objection [c]) (in the example presented above this third variable was the patient's putative fear of, and desire to control, his own anger, this being what on this account is supposed to produce, via projective mechanisms, both the patient's distress as regards cleaning tasks and the like and his style of dealing with such tasks).

What, then, if it could be shown that ex-patients continue to exhibit the cognitive style postulated by Reed (still assuming for the sake of the present argument here that obsessional patients also exhibit this style)? Would this observation confirm Reed's thesis, and would it answer the objections considered above? It would seem not. This finding would certainly be consistent with Reed's position - as noted earlier, he suggests that the cognitive style of obsessionals is a permanent feature of their functioning, obsessional disorders resulting from the exacerbation of this feature (although note that Reed's position would thus be inconsistent with patients and ex-patients exhibiting exactly the same degree of the cognitive style featured in his hypothesis.)

Two of the objections considered above would, furthermore, have some difficulty in explaining this finding with ex patients. Thus, objection (c) would at least have to explain why the third variable it postulates should continue to produce the same cognitive style in ex-patients which they exhibited while patients, but not continue to cause the distress they formerly suffered (this distress supposedly being, on this objection, a co-effect of this same variable). Objection (a) would have still more difficulty in explaining this observation. Why should ex-patients continue to be anxious as to such matters as their performance on experimental tasks being judged - the explanation this objection provides of the ex-patient's

performance on such tasks - if ex-patients are no longer being troubled by anxieties concerning such matters as regards their everyday lives?

But objection (b) would require at most minor alterations to accommodate the same observation. Thus, it could explain this cognitive style in ex-patients as having generalised from the obsessional difficulties they once had, just as in the original version of objection (b) this style was explained as having generalised from the present difficulties people who are currently patients have.

To sum up, then, the results from experimental tasks such as Reed's could not provide strong confirmation of his account. Whether these tasks were to be conducted with obsessional patients or ex-patients - although particularly in the former case - explanations other than Reed's would be available to explain why the thinking style he postulates had been observed on such tasks. These tasks clearly have a greater potential to disconfirm Reed's thesis. Strong evidence against his account would be provided by a failure to observe the thinking style he hypothesises in the performance of either obsessional patients or ex-patients.

Continuing to assume for the sake of argument, then, that the cognitive style featured in Reed's account is exhibited by obsessional patients, what would be reasonable evidence in support of the claim that this style is causing the distress of these patients as regards checking and cleaning tasks and the like?

One important implication of this claim is that a reduction in the thinking style exhibited by these patients would in turn lead to a reduction in the distress experienced by them. Investigating whether or not this implication is true would therefore be a crucial test of Reed's thesis, and a test which, furthermore, could potentially support Reed's account while disconfirming all three of the objections considered above. Thus, in their various ways, these objections suggest that the thinking style Reed hypothesises obsessional patients to have is either an effect of their distress, or its co-effect. All of these objections therefore claim, with Reed, that the thinking style and distress of these patients are associated with one another while denying, against Reed, that this association consists of this distress being produced by the thinking style. This is, then, why running obsessional patients through experimental tasks such as Reed's fails to answer any of these objections - this could at most only establish what these objections themselves assert, that the distress of these patients is associated with the thinking style featured in Reed's account. None of these objections, by contrast, could readily make sense, as Reed's account could, of manipulations of this thinking style leading to changes in the level of distress experienced by obsessional patients. If this thinking style is an effect of the patient's distress, or its co-effect, changes in that thinking style should have

no effects on the patient's distress (or indeed be impossible without such effects first being brought about). A similar approach would be to attempt a reduction in the obsessional thinking style of ex-patients (assuming here that there are ex-patients who continue to exhibit this style). The crucial observation would be whether the reduction or elimination of this continuing tendency to exhibit this style helps to prevent the subsequent reappearance of obsessional difficulties.

Studies such as those suggested above require some means by which to change the hypothesised thinking style of obsessional patients and ex-patients. The therapeutic interventions Reed derives from his hypothesis are one way in which this could be attempted. Given the theoretical importance of such attempts, therefore, the absence, as noted above (section 4.1.2), of treatment outcome data for these interventions in Reed's (1985) discussion is especially regrettable.

Even these studies could not provide conclusive evidence that the thinking style of obsessional patients causes their distress. The finding that changing the thinking style of patients reduces their distress would only show that this style helps maintain this distress; the finding that changing the thinking style of ex-patients reduces their rate of relapse would only show that this style helps produce the reappearance of distress. But either of these findings would at least make it more plausible to claim that this thinking style causes the distress of obsessional patients, and would in any case be of clinical value.

To sum up, then, the results from experimental tasks such as Reed's could in themselves only provide strong evidence against his theory. If one intended instead to provide strong evidence in favour of this theory, the major justification for running such tasks would be that they might provide evidence for the presence of the thinking style Reed postulates, which could then be manipulated in studies such as those suggested above.

4.8.4 Non-experimental lines of evidence for Reed's account

The foregoing arguments do certainly not mean that there could be no grounds for believing Reed's thesis in the absence of any evidence concerning the outcome of attempts to change the thinking style outlined in that thesis. Reed himself attempts to provide such grounds with his suggestions that compulsive behaviour increases the discomfort of obsessional patients and that the nature of this discomfort - depression, anger and the like - is such that it may be very plausibly attributed to the frustration of carrying out these behaviours. While these observations may be disputed (see sections 4.10 and 4.11) they certainly would, if sound, have provided evidence in favour of Reed's analysis.

4.9 Reed's account, and the definition of the obsessional personality and personality disorder

As noted in section 1.2.2, Reed argues that there are clear diagnostic criteria for OCD. As regards the obsessional personality and personality disorder, however, Reed says that "no clear operational definition of [this] has yet evolved" (1985, p44) and attempts no definition himself, presenting instead thirty-three traits, such as punctiliousness, indecisiveness and thoroughness, which are most commonly accepted as being among the constituents of this personality/personality disorder.

Yet there is some tension between Reed's theoretical account, and this position regarding the definition of the obsessional personality and personality disorder. Reed's review of the thirty-three traits most commonly associated with this disorder is followed by his account of these traits in terms of difficulties in "spontaneously structuring", and the tendency to "overstructure", this being presented as a "psychological/semantic synthesis" (p114, emphasis not in original). But if Reed believes that his account picks out a semantic link between all obsessional personality traits, why does he not attempt a definition of the obsessional personality disorder as one in which traits exhibiting the features picked out by this account predominate?

4.10 The Experience of compulsion

As Reed believes OCD to be a pathological extension of the traits of the obsessional personality disorder, why does his psychological/semantic synthesis of these traits also appear to play no part in his account of the defining criteria of OCD (see section 1.2.2)? Reed's reply to this would probably refer us to his account of what it is for an experience to have "a subjectively compulsive quality". This quality is, in Reed's view (1985, p5), the most fundamental of the three defining criteria of OCD he puts forward, and his analysis of it turns out to be closely related to his "psychological/semantic synthesis". Quoting several of his patients' accounts of this experience, he suggests that for the most part these accounts do "not stress the positive strength of the thoughts, but the negative strength of [the patient's] will-power" (1985, p129). The experience is characterised, Reed suggests, "not so much by an awareness of a powerful, compelling force as by a feeling of inadequacy in volitional process. The problem seems to be related not to a pathological intensity of excitation, but to a relative failure of inhibition" (1985, p129, original emphasis).

Reed offers some harsh criticisms of theorists - including both behaviourists and psychoanalysts - whose approaches do emphasise, in Reed's terms here, "the power of the thoughts" experienced by patients. The belief in powerful, compelling forces which direct the

action of an individual against his own will makes sense most readily, Reed tells us, in a culture which believes in "malign or punitive spirits" (1985, p121). This leads Reed to suggest that "it is only a slight exaggeration to claim that contemporary views of obsessional experience savour of medieval thinking" (1985, p122). Reed suggests instead that the experience of compulsion may be seen as an "imbalance" or "malfunction" in "negative feedback", that is, as the poor "spontaneous structuring of experience" detailed in his hypothesis.

Reed's criticisms of accounts which suggest there to be "powerful forces" compelling obsessional patients are certainly unfair. All that this suggestion amounts to is the claim that the sufferer experiences extreme emotion and/or feels highly motivated to act in a certain way. No belief in malign spirits is implied.

There are in any case difficulties for Reed's account of the experience of compulsion. If this experience amounts to a "failure of inhibition" in Reed's sense - an "imbalance" in "negative feedback" - then for a thought or action to be attended by this experience this thought or action would have to occur or be performed repetitively - it is this which would be argued by Reed to reflect the patient's failure to inhibit the thought or action. Yet there is no necessity for compulsive thoughts or actions to be repetitive - as noted earlier (see section 4.4), for example, Rachman and Hodgson (1980) report that compulsive cleaning is often performed without any repetitiveness or doubting.

Commenting on Rachman and Hodgson's observation that some obsessional patients have no difficulty in deciding when to stop their compulsive behaviour, Reed suggests that the problem of these patients remains one of deciding to act upon their decisions - according to Reed these patients can, that is, decide when they should stop but fail to do so (Reed 1976). But this argument surely misses the point - these patients are reported by Rachman and Hodgson to carry out short rituals, so they evidently can act on their decisions to stop.

It also seems that Reed's account of obsessional disorders contradicts his analysis of the "experience of compulsion" by virtue of an implication of his account which Reed himself does not acknowledge. This implication is that compulsive behaviours need not always be repetitive (his account appears similarly to imply that all such behaviours need not always increase discomfort). Reed's account does suggest that compulsions should tend to be both discomfort-increasing and prolonged as the patient struggles to "structure" any task to his own satisfaction. But having arrived - by "overstructuring" the task - at what he regards as a proper or satisfactory way of carrying it out, Reed's account surely allows that subsequent performances of that task need not involve prolonged, discomfort-increasing behaviour. Thus,

Reed suggests that thoroughness in the carrying out of some behaviour is one way in which a task may be overstructured (Reed 1985, p55). And the thorough performance of some behaviour need not involve that behaviour being prolonged and accompanied by doubts (it is similarly consistent with that behaviour reducing the discomfort of the person who carries it out). For example, someone may be said to be thorough in keeping a table clean merely by removing any slight blemish or speck of dirt from it as soon as it appears - no repetitiveness or doubting in the performance of this removal is implied.

When one turns to the obsessional personality and personality disorder still further tensions arise between Reed's account of obsessional disorders and his analysis of the experience of compulsion. Reed would argue the traits of this personality and personality disorder not to involve the experience of compulsion at all - it is the absence of this experience which, in Reed's view, helps to distinguish these traits from obsessional symptoms. Yet according to Reed these traits involve - albeit to a lesser degree that obsessional symptoms - the poor spontaneous structuring of experience (the "malfunction in negative feedback") which is central to Reed's analysis of the experience of compulsion.

4.11 The role of anxiety

Reed draws attention to an important point when he stresses that anxiety is not the only mood disturbance observed in obsessional disorders; other workers writing from various perspectives make the same point, for example Beech and Liddell (1974), Beech and Perigault (1974), Rachman and Hodgson (1980), Claridge (1985). While all of these authors argue this point, as Reed does, on the basis of phenomenological evidence, other kinds of data have also been used to defend the related suggestion that OCD should not be classified as an anxiety disorder. Fineberg (1990), for example, points out, in defence of this claim, that OCD differs from anxiety disorders in several (non-phenomenological) ways (see section 3.3.6.2).

Reed nonetheless appears to understate the role played by anxiety in obsessional disorders and in particular, his assertion (see section 4.1.1) that the anxiety of obsessional patients only concerns such questions as "will I ever be well?", not questions such as "am I dangerously contaminated?", is surely implausible.

4.12 The obsessional personality/personality disorder, OCD and non-obsessional symptoms

As pointed out earlier, Reed's account requires OCD only to arise in the context of an obsessional personality or personality disorder. This does not square with current findings, for example Black (1974) and Pollack (1979, 1987). (This raises the interesting and little researched question of how, if at all, the obsessional symptoms of patients who do have

obsessional personalities differ from those of patients with other personality types.)

Reed is explicit that his account of obsessional disorders treats the mood disturbance observed in them as secondary, and thus sharply distinguishes them from depressive and anxiety disorders. How does Reed make sense, then, of the link between obsessional and depressive disorders, given Lewis' (1936) claim that this link cannot to be explained in terms only of the depressing nature of obsessional disorders? Support for Lewis' position is provided by Gittleson's (1966) observation that some patients experience an emergence or increase in obsessions only once they have already become depressed. Reed (1985, p143) suggests that this observation may reflect a "spreading effect" - with a general lowering of resistance in depression, he argues, a wider range of obsessions may intrudc. But why should one suppose that "low resistance" leads to the intrusion of obsessions? (This appears to be an especially surprising claim for Reed to make, given his diagnostic criterion that all obsessions must be resisted.)

Reed's account also has difficulty in explaining the co-morbidity of OCD with anxiety disorders such as simple phobias, social phobias, agoraphobias, panic disorder and separation anxiety (Rasmussen and Tsuang [1986]). This finding suggests there to be some overlap in the mechanisms which produce these various disorders, yet Reed's account explicitly leaves no room for such an overlap.

4.13 External structuring

Reed (1968) reports that the compulsive behaviour of some obsessionals is reduced by the presence, reassurance and/or intervention of a trusted person. This enables the patient, Reed (1968, 1985) points out, to delegate decisions and responsibilities. But what Reed does not explain is why this delegation of decisions and responsibility should, on his account, be of help. The problems which Reed suggests are encountered by obsessionals in determining the answers to questions such as "Did I do it properly?", should be no greater than those encountered in answering questions such as "Has this other person done it properly?" (Jakes 1987).

Indeed, rather than supporting Reed's account, the observation that obsessionals benefit from the delegation of decisions and responsibilities would suggest their problems have instead to do with <u>personal uncertainty</u>, that is, doubts specifically about themselves and their own action. (It could similarly be argued that such doubts are also present in the contents of some obsessional symptoms, for example those in which the patient checks or ruminates as to whether or not he has performed some violent act [Jakes 1987]).

This phenomenon of being helped by the presence, reassurance and/or intervention

of a trusted person is in any case only exhibited by some obsessionals (Stern and Cobb 1978, also see Chapter 2). It would be of interest to know if this feature bears any relationship to Lewis' (1936) distinction, discussed earlier, between obstinate and irritable obsessional personalities on the one hand and vacillating and submissive obsessional personalties on the other. Is it only the latter who tend to seek the reassurance and help of others? And does the obstinacy of the former group tend to produce precisely the opposite profile, of having to do things for oneself and being unable to trust others to do them properly? Evidence of possible relevance to these questions is provided by Rachman and Hodgson (1980, p54) who report checkers to be both more likely than cleaners to seek reassurance and also more likely than cleaners to have a vacillating and uncertain personality profile.

4.14 Reed's account of Janet

Reed argues that his formulation of obsessional disorders has much in common with Janet's account of psychathenia. Some comments on this are offered elsewhere (section 3.4.5), but it is worth noting here that, among other difficulties facing this interpretation, is the fact that Janet included various disorders in addition to OCD under the common rubric of "psychasthenia", including what would nowadays be diagnosed as agoraphobia, social phobia and panic disorder (see section 3.4.2), and endeavours to explain all of them, along with OCD, in terms of a lowering of the sufferer's "psychological tension" (see section 3.4.4). This is in clear contrast, then, to Reed's own account, which is intended to apply specifically to obsessional disorders, and is unable to explain, as noted above (section 4.12), the co-morbidity of OCD with these other disorders.

4.14 Summary

Reed originally argued that obsessional disorders all stem from difficulties in the "spontaneous structuring" of experience, leading to compensatory "overstructuring". Reed himself later pointed out that this hypothesis as it stands makes poor sense of at least some cases, especially, it seems, those involving checking and rumination. He introduced the redintegration supplement in an attempt to remedy this but this supplement turns out to be implausible. Reed's hypothesis has also been argued to make poor sense of other kinds of obsessional difficulties in which doubts, indecision and ritualising do not play a major role or do appear at all. Certain aspects of some obsessional difficulties which do involve these features, furthermore, are not explained by this account, in particular, certain themes tending to feature in many cases, and these difficulties sometimes involving just a few areas of the patient's life.

It has been argued that Reed's hypothesis also fails to explain the motivation of

obsessionals in carrying out their compulsive behaviour. A revised version of Reed's thesis was suggested which can explain why at least some obsessionals are highly motivated to behave as they do, but this thesis fails to account for various observations, once again especially those which tend to be associated with checking difficulties. It was argued that a partial explanation of such difficulties from this perspective may be possible, however. This "revised thesis" was also argued to be poor at distinguishing obsessionals from other psychiatric groups, including phobic and deluded subjects.

The type of experimental evidence Reed has presented in favour of his thesis has been argued to provide only weak support, while the clinical observations Reed cites to this same end are at best controversial. Objections have been brought against Reed's comments as regards the use of behavioural and cognitive therapy and assertiveness training with obsessionals.

It has also been argued that Reed's account misrepresents the role of anxiety in obsessional disorders and carries implausible implications as to the relationship both between OCD and the obsessional personality/personality disorder and between OCD on the one hand and anxiety and depressive disorders on the other.

A number of further comments have been offered as regards Reed's discussion of "external structuring", his interpretation of Janet, his account of the experience of compulsion and his comments on the definition of OCD and of the obsessional personality and personality disorder.

What of Reed's account may be salvaged from the foregoing discussion? The account appears to be most plausible when regarded as a partial explanation of some obsessional difficulties, in particular those where one observes the patient having, in Reed's terms, problems "structuring" some task, and tending to perform it in a "overstructured" manner. Among the best examples of cases of this kind appear to be at least some instances involving contamination fears and cleaning behaviour (see the example of handwashing discussed in section 4.1.1 above).

The "revised thesis" seems most likely to provide a full explanation in the case of those obsessional difficulties where some unlikely outcome is feared by the patient, but neither constant doubts about, nor endless repetitions of, compulsive behaviours are observed. Many cases involving cleaning difficulties correspond to this profile.

Both Reed's account and the revised thesis, therefore, seem to be most plausible in the case of at least many instances of cleaning difficulties. It needs to be stressed, however, that the greater plausibility of these approaches as regards cleaning problems really amounts

to no more than the absence of the objections which arise regarding the application of Reed's account and the "revised thesis" to other kinds of difficulty, such as checking behaviours and discrete obsessional thoughts, images and impulses. There are, that is, no positive grounds for accepting either of these approaches even in the case of cleaning problems, and it remains entirely possible, therefore, that the cognitive factors reviewed above will turn out to be entirely secondary phenomenon in obsessional disorders, playing only a minor, or even no role, when they appear at all, in producing the pathological thinking and behaviour which is observed.

In Part C, three empirical tests of Reed's account, and the "revised thesis", are reported, along with reviews of the earlier studies which have employed these three tasks.

Part C: Experimental Investigation of the Cognitive Characteristics of Obsessive Compulsive Disorder patients

Chapter five: Two tasks involving "conceptual structure"

5.1 Introduction

Reed (1985, 1991) has reviewed the empirical investigations of the cognitive characteristics of OCD patients and patients with obsessional personality disorder (as in the previous chapter, these two kinds of patient will be referred to collectively as "obsessionals" in what follows). Reed (1991, p77) emphasises that there have been "only a very limited number of empirical, data based reports" investigating these characteristics and he notes that these reports have been restricted to five areas of cognitive functioning, these areas being termed by him "conceptual structures" (p81), "reasoning" (p85), "indecision and uncertainty" (p86), "memory" (p90) and "imagery and redintegration" (p93). Reed (1991) suggests that "such slim empirical evidence as has emerged...suggests that...obsessional people manifest a characteristic cognitive style" (p96), this style being, Reed suggests, that which is described in his (1985, 1991) account (see Chapter 4).

In the present investigation, three of the experimental tasks included in Reed's review were used. Some comments will be offered as to some of the strengths and weaknesses of these tasks, and previous findings with them will be examined. The choice of the three tasks included in the present investigation does not imply that previous investigations in which these tasks have been used are more important than the other investigations included in Reed's review. The claim is only that the tasks chosen are at least as important as any of these others (see Rachman and Hodgson [1980, ch 13] for some criticisms of some of these other studies). Judgment is furthermore suspended as to how far the findings of these other investigations which Reed argues to be generally consistent with his account (1) would bear more detailed scrutiny, (2) would prove to be replicable in further investigations and (3) are related to the three tasks which have been subjected here both to such scrutiny and to attempted replication.

The two tasks discussed in the present chapter below are presented by Reed (1991) as investigating the "conceptual structures" of obsessionals, that discussed in next chapter is presented by him as an investigation of their "indecision and uncertainty".

5.2 Persons and Foa's (1984) investigation

5.2.1 The questions investigated by Persons and Foa

Persons and Foa (1984) investigated OCD patients with what Reed (1969[ii]) describes as an "inductive classificatory" task, that is a task in which the subject is given an array of items, the objective being for him or her to determine the categories or classes into which they

can be most appropriately organised (see section 4.1.1). Reed himself (1969[ii]) has employed a different "inductive classificatory" task, and reported that the obsessional subjects he studied (all of whom had obsessional personality disorders) performed on it as his account predicted they should (1969[ii]) (see section 4.1.1). Frost et al (1988) and Fuller (1985) have since used Persons and Foa's task in further investigations (see sections 5.3.1 and 5.3.2 respectively).

Persons and Foa attempted to test two questions, the answers to both of which are relevant to Reed's (1985) account. The first of these questions was "whether the concepts of obsessive-compulsives are excessively complex and over-specific...or, conversely, whether they are excessively simple" (1984, p259); the second question was "whether the cognitive mechanism underlying obsessional fears are [sic] specific to the content of the fear, or whether they reflect a general deficit in information processing" (1984, p259).

Persons and Foa's "complex concepts" hypothesis is very like Reed's account of obsessional disorders, a discussion of which is offered elsewhere (Chapter 4). Thus, Persons and Foa's "complex concepts" hypothesis suggests, like Reed's account, that "in order to decide whether a given object is contaminated, for example, the patient must make detailed observations of all aspects of the object to acquire the information needed to make fine discriminations between degrees of contamination" (Persons and Foa, 1984, p259). (Persons and Foa neither affirm nor deny, in setting out this hypothesis, Reed's claim that this cognitive style is the primary dysfunction in OCD - see Chapter 4.)

Persons and Foa's alternative hypothesis, that OCD patients are characterised by "simple concepts" is the opposite of this position. Taking as their example once again OCD patients with contamination fears, Persons and Foa suggest in support of the "simple concepts" hypothesis that "...degrees of contamination do not seem to exist for the obsessive compulsive. Objects are never slightly, partially or somewhat contaminated; they are either contaminated or not". Persons and Foa argue that "this sort of mechanism might account for the clinical observation that an obsessive-compulsive categorises many more objects as contaminated than...[others] do" (p259-60).

But this is weak support for the "simple concepts" hypothesis. The claim that degrees of contamination do not exist for some OCD patients could surely only be based upon the observation that such patients want to have contact only with those things they regard as completely clean. But this observation is quite consistent with the hypothesis that these patients have "complex concepts" of contamination - degrees of contamination defined in a highly complex way may exist for a patient who exhibits complete avoidance with respect even to those situations and objects he regards as only very slightly contaminated. Similarly,

Persons and Foa's observation that an OCD patient may classify a large number of things as contaminated is perfectly consistent with that patient's concept of contamination being highly complex. Indeed, an OCD patient might avoid and/or have his or her compulsions provoked by many situations and objects merely because he or she is uncertain whether or not these situations and objects are contaminated - and this would contradict rather than support Persons and Foa's "simple concepts" hypothesis.

Persons and Foa further suggest that their "simple concepts" hypothesis is "reminiscent of the claim that the conceptual structure of obsessive-compulsives is more "monolithic" and less "articulated" than that of normals" (p260) and cite the work of Makhlouf-Norris and her co-workers (Makhlouf-Norris et al 1970) in support of the claim that OCD patients have such "conceptual structures". But Reed cites these same findings (1985, p191) in support of the "complex concepts" hypothesis while Persons and Foa themselves note that Millar (1980) failed to replicate Makhlouf-Norris's results. Given all of these difficulties for the "simple concepts" hypothesis, therefore, it will not be considered in any further detail in what follows.

5.2.2 Persons and Foa's task

Persons and Foa presented their subjects with four packs of 3" by 5" index cards, each pack containing 20 cards. Each card carried the name of an object or a brief description of a situation. Subjects were asked to sort each of these packs into groups of those cards bearing the names of objects and/or the descriptions of situations which the subjects regarded as being the same with respect to a given concept. Two of the packs were sorted in terms of "neutral" concepts ("size" and "temperature"), that is topics which were unlikely to feature in the contents of OCD symptoms, while the other two packs were sorted in terms of "feared" concepts ("contaminated or dirty" and "seriousness of mistakes"), that is topics which were in contrast likely to feature in these contents. The time taken and number of categories used to sort each of these packs of cards were recorded. The packs were presented in the order "size", "contaminated or dirty", "temperature" and "seriousness of mistakes" (see section 5.5.2.2 below for the items in each of these packs).

5.2.3 Persons and Foa's predictions and results

The "complex concepts" hypothesis, Persons and Foa suggest, predicts that OCD patients, as compared with controls, will both form more categories and require more time to sort out each of the packs (see section 4.1.1 for a discussion of the same prediction as made by Reed's account regarding "inductive classificatory" tasks in general). The "simple concepts" hypothesis makes the opposite prediction that OCD patients will need fewer categories and less time.

As regards the question of whether the putative distinctive cognitive style of OCD patients is specific to their symptom contents or rather appears in all areas of their functioning, Persons and Foa argue that Reed (1969[i], 1969[ii]) has only tested this "indirectly, by showing that obsessive-compulsives differ from non-obsessives in performance on a number of neutral tasks" (1984, p260) (his results thus suggest the cognitive style in question to be general to the functioning of these patients, on Persons and Foa's argument). Persons and Foa suggest that their card sorting task, by contrast, tests this question "directly", because of the inclusion of cards featuring both "feared" and "non-feared" items. The hypothesis that the putative distinctive cognitive style of OCD patients is specific to their symptom contents predicts that the performance of these patients will differ from controls only with respect to the feared packs, Persons and Foa suggest. The hypothesis that this distinctive style is general to the functioning of OCD patients predicts, Persons and Foa argue, that the performance of these patients will differ from that of controls with respect both to the "feared" and "non-feared" packs. Reed's account appears to predict, with Persons and Foa's "general style" hypothesis, that there should be no specific effect with the "feared" cards, although a few of his remarks [see section 4.6.2] may be consistent with this finding.

Persons and Foa found that their "obsessive-compulsives" (seven subjects who were either currently or formerly diagnosed as suffering from OCD and currently scoring 10 or more on the Maudsley Obsessive-Compulsive Inventory [MOCI], Hodgson and Rachman [1977]) did indeed use both more categories and time as regards both the neutral and feared packs than controls (eleven psychiatric patients who had never been diagnosed as suffering from OCD and currently scoring less than 10 on the MOCI). These findings indicate the obsessive-compulsive group to have "a general deficit", Person and Foa suggest, although they also note "an additional impairment in [the obsessive-compulsive's] sorting speed that is specific to the feared items" - the obsessive-compulsives, in contrast to Persons and Foa's controls, took "much more time to sort the feared items than the neutral ones" (1984, p263). Persons and Foa suggest that a specific deficit is therefore also indicated - although they note, as Reed (1991) too has argued in connection with their results, that this finding might "be viewed as simply an exaggeration of the general deficit" (1984, p263) as regards the "feared cards". Persons and Foa also note that their obsessive-compulsives were more depressed than their controls, and suggest (1984, p262-3) that additional studies are required to determine whether this difference might explain some of their findings. The study had no normal control group.

There is a hint in Persons and Foa's paper that all of the OCD patients they examined

may not have exhibited the same profile of results as that observed in the case of those subjects who were included in their obsessive-compulsive group. Thus, Persons and Foa report (1984, p261) that they excluded 10 "borderline cases" who were tested, such cases being defined as either (1) those who were diagnosable as OCD (or had been so diagnosed in the past) with MOCI scores less than 10, or (2) those control subjects who had MOCI scores of 10 or more. But if one is looking to this investigation as a test of a proposed explanation of OCD such as Reed's, it is unacceptable to regard OCD patients as "borderline cases" merely because they have low MOCI scores. This is particularly so because such patients may well exhibit OCD symptoms in only one or two restricted areas of their functioning - this being what keeps their MOCI scores low despite their experiencing clinical levels of distress - and are thus especially difficult cases for an account such as Reed's to explain (see section 4.6.2). If such patients were therefore a large enough proportion of the original sample of OCD patients examined, their exclusion from the study would weaken it considerably as a test of Reed's account. (Such patients would similarly be of interest from the point of view of the question, raised by Persons and Foa themselves, of whether the cognitive deficits exhibited by OCD patients are specific or general.) Unfortunately, the data from the "borderline cases" are no longer available (Persons 1988), so it is impossible to know how many of these cases were OCD patients or what difference, if any, their inclusion would have made to Persons and Foa's findings.

5.3 Other investigations using Persons and Foa's task

5.3.1 Frost et al's (1988) investigation

Frost et al (1988) employed Persons and Foa's task to investigate a group of "non-clinical compulsives" (nine normal subjects with MOCI scores of 10 or more). Their control group were "non-compulsives" (fifteen normal subjects with MOCI scores less than 5). It was found that the "non clinical compulsives", like Persons and Foa's obsessive-compulsive group, took more time to sort the packs of cards than the "non-compulsives". However, unlike Persons and Foa's obsessive-compulsive group, Frost et al's "non-clinical compulsives" did not require more categories than their "non- compulsives" to sort the packs, and both groups took more time to sort the "feared" than they did to sort the "non-feared" packs of cards. Frost et al also report that it was high scores on the checking rather than the washing subscale of the MOCI which were associated with subjects taking more time to sort the packs of cards.

Frost et al suggest that their results provide limited support for the complex concepts hypothesis among non-clinical compulsives, and argue that their failure to replicate Persons and Foa's "number of categories" effect may be due to the use of a non-clinical population

in their study. They further suggest that the specific effect for time in Persons and Foa's study may be due more to Persons and Foa's control group than to the obsessive compulsives. Frost et al tentatively suggest that "overspecificity may characterise and/or be important for the development of compulsive checking but not compulsive washing". Theoretical considerations (see sections 4.1.2, 4.2 and 4.5), however, present difficulties for this view, suggesting that Reed's account and the "complex concepts" hypothesis cannot be plausibly applied to at least many instances of checking difficulties.

5.3.2 Fuller's (1985) investigation

A third study using Persons and Foa's task (not included in Reed's [1991] review) is reported by Fuller (1985). The experimental group in this investigation consisted of eight psychiatric patients with MOCI scores of 10 or more; controls were a group of eleven normals and a group of nine psychiatric patients with low MOCI scores. There were no group differences with respect either to the number of categories required or the time taken to sort the packs of cards. There was also no difference between the time taken to sort the "feared" and "non feared" packs of cards, although all groups required fewer categories to sort the "feared" than they required to sort the "non feared" packs of cards.

5.4 The investigations by Persons and Foa, Frost et al and Fuller - conclusions

There are several conclusions to be drawn concerning the results from these studies, and a number of questions to which these results give rise. Firstly, these studies do not provide very strong confirmation that OCD patients perform on Persons and Foa's task as Reed's account or the "complex concepts" hypothesis would predict. The only study with an experimental group composed of subjects who had received a diagnosis of OCD also excluded the results of an indeterminate number of such subjects. The complete or partial failure to confirm Reed's predictions in the other studies are at least equally inconclusive as evidence against Reed's account of OCD. Thus, although Frost et al (1988, p276) suggest that "analogue populations...may provide valuable insights into obsessive-compulsive disorders", studies using such populations can really only raise, not answer, questions concerning these disorders.

Secondly, several questions are indeed raised by Frost et al's study with their analogue population. Do some OCD patients, like Frost et al's non-clinical compulsives, require more time but not more categories than controls to sort (some or all of) Persons and Foa's packs, thus only partially confirming Reed's account? And among OCD patients, again like Frost et al's "non clinical compulsives", are those with checking difficulties distinguishable from those with cleaning difficulties as well as from non OCD controls in terms of the number of

categories they use and/or the time they take to sort the packs? Or is it rather, as theoretical considerations suggest may be somewhat more likely (see section 4.3.1.2, 4.2 and 4.5), that patients with cleaning difficulties are distinguishable in these ways from non OCD controls and patients with checking difficulties? Either way, such results would suggest that Reed's account applies at most to some but not all OCD patients.

Thirdly, while Persons and Foa's experimental group contained both OCD patients and ex-OCD patients, none of the investigations which have used Persons and Foa's task has included a group composed exclusively of the latter type of subject. Yet the results of ex-OCD patients on such tasks are of importance to Reed's account of obsessional disorders, as a test of his claim that the cognitive style described by his account is a permanent feature of the functioning of people disposed to develop OCD (see section 4.1).

Fourthly, the findings concerning the performance of subjects on the "feared" as opposed to the "non feared" packs of cards in Persons and Foa's task are contradictory. Frost et al found that all of their subjects took longer to sort the "feared" than the "non feared" packs of cards but used the same number of categories to sort each type of pack. Fuller found the reverse. All of Persons and Foa's subjects, like Fuller's, required fewer categories to sort the "feared" than the "non feared" pack, but only their OCD patients required more time to sort the "feared" pack.

5.5 The present study

5.5.1 The questions investigated in the present study

The present investigation was undertaken to answer several of the questions which have been raised or unanswered by these earlier studies, in particular:

(i) Is the performance of OCD patients on Persons and Foa's task distinguishable from that of psychiatric and normal controls, and if so is this in terms only of the time taken by OCD patients to sort the packs or in terms of both this and the number of categories they use?

(ii) Are those OCD patients whose major difficulties involve checking behaviour distinguishable in either or both of these ways not only from non OCD controls but also from those OCD patients whose major difficulties involve only cleaning? Or is it the patients whose major difficulties involve cleaning who are distinguishable in either or both of these ways both from non OCD patients and from patients whose major difficulties involve only checking?

(iii) Are ex-OCD patients distinguishable from (some or all) OCD patients and/or from non OCD controls in terms of their performance on Persons and Foa's task?

(iv) Are there any differences between the number of categories used and/or the time taken by subjects to sort the "feared" and "non feared" packs of cards? Are any such

differences specific to (some or all) OCD patients?

5.5.2 Method

5.5.2.1 Subjects

Patients were recruited from the Psychological Treatment Unit at the Maudsley Hospital; London and were selected, along with normal subjects chosen from the secretarial, nursing, medical and other staff at the Maudsley Hospital, such that the following groups were formed. No patient approached refused to take part in the study.

All of the patients and ex patients had been diagnosed by a senior psychiatrist; the OCD patients (groups 1, 2 and 6) and the ex OCD patients had all received a DSM III-R 300.30 classification. The type of symptom reported by these patients (checking, cleaning or other) was ascertained in an interview conducted by ICJ.

Group 1: Cleaners

10 OCD patients (6 female, 4 male) with a mean age of 37.4 years (SD=13.58 years) whose major difficulty involved the cleaning or washing of self, others and/or objects etc. and who reported no major difficulties which involved checking.

Group 2: Checkers

10 OCD patients (5 female, 5 male) with a mean age of 38.30 years (SD=11.82 years) whose major difficulty involved the checking of plug sockets, switches and/or taps etc. These subjects were selected for the presence of checking difficulties, rather than for both this and the absence of cleaning difficulties. Two of the subjects in this group reported major cleaning difficulties in addition to their checking problems.

Group 3: Ex-OCD patients

10 subjects (6 female, 4 male) with a mean age of 38.2 years (SD 11.99 years) all of whom (1) had formerly been diagnosed as suffering from OCD, (2) had been treated with behaviour therapy (as well as with drugs, in a few cases) and (3) were no longer reporting any difficulties of clinical severity. Five of these subjects had formerly been cleaners, three checkers; two had difficulties which could be classified in neither of these ways, for example repeating actions to remove bad thoughts.

Group 4: Anxious controls

10 patients (6 female, 4 male) with a mean age of 38.6 years (SD=12.63 years) diagnosed as suffering from social phobia (DSM III-R 300.23) or agoraphobia (DSM III-R 300.22), who had not been diagnosed as suffering from OCD (DSM III-R 300.30) and who did not report any obsessive-compulsive difficulties.

Group 5: Normal controls

10 subjects (6 female, 4 male), who had at no time sought psychiatric help, with a mean age of 36.3 years (SD=10.98 years).

Groups 6: Non checking/cleaning OCD patients

3 OCD patients (2 female, 1 male) with a mean age of 39.67 years (SD=13.61 years) whose symptoms involved neither cleaning nor checking behaviour.

All patients had been ill for at least one year and reported no psychotic, severe affective or physical illness to clinical staff. Those patients who were receiving medication had been on a stable dose for at least six months; no ex patient was receiving medication. The twenty three OCD patients in this study were also included in the investigation of the characteristics of obsessive-compulsive experience reported elsewhere (Chapter 2). All subjects were aged between 18 and 60 years.

5.5.2.2 Materials and measures

The materials used were the same as those described in Persons and Foa (1984). The items in each of the four packs were as follows:

"Size" pack: a gold coin, a diamond, Atlantic Ocean, the planet Venus, a rose, an atom, a magazine, a snowflake, a piece of stationery, a wristwatch, an obese person, a cat, a newborn baby, an elephant, a bowl of soup, a hummingbird, a ranch in Texas, a swimming pool, an ocean liner, a cinema.

"Contaminated or Dirty" pack: floors, perspiration, a library book, money, sterilized water, poorly dressed people, furniture surfaces, a person who just had a shower, a hospital, doorknobs, sterilized surgical instruments, the seats on a public bus, Kleenex, radioactive materials, a mouldy piece of cheese, faeces, urine, a toilet seat in a public convenience, garbage cans, dog faeces.

"Temperature" pack: a book, a wooden chair, a dog, a sauna, a campfire, dry ice, a pencil, the inside of a refrigerator, an igloo, a man, a fish, the sun, a large lake, a newborn baby, a gold necklace, Antartica (air temperature), a glacier, a sun lamp, a cracker, a bowl of ice cream.

"Seriousness of Mistakes" pack: cooking carelessly and starting a fire at home, not understanding something you read, being five minutes late for an appointment, causing a car accident, forgetting to put on the brake when parking your car, losing some money, going to bed at night without unplugging electrical appliances, paying a bill a few days late, making a typing error, leaving sharp scissors around when young children are in the house, forgetting to lock your house, losing your temper and shouting, burning a cake, forgetting to lock your car, wearing unmatched socks, leaving your umbrella on the bus, giving inaccurate directions,

forgetting to serve the bread at a dinner party you give, forgetting to turn off the stove, failing to return a telephone call.

The order of items within each pack of cards for all subjects in the present investigation was that given here. The number of categories used and the time taken by subjects to sort each of these packs of cards were recorded. Before sorting these four packs of cards, the subjects were given a practice pack containing ten cards. The items named on these were to be sorted in terms of their weight. (The results from this practise pack are not reported.) The items in this pack were: a magazine, a snowflake, a hummingbird, an atom, a pencil, money, a book, the seats on a public bus, garbage cans, a man.

Subjects were also required to complete the following control measures: Beck Depression Inventory ("BDI", Beck et al 1961), Cognitive Failures Questionnaire ("CFQ", Broadbent et al 1986), "Doubting, Repetition and Relief" questionnaire ("DRR", Rachman and Hodgson 1980, pp125-136 - this title for the questionnaire is not used by Rachman and Hodgson themselves), Eysenck Personality Questionnaire ("EPQ", Eysenck and Eysenck 1975), Maudsley Obsessive-Compulsive Inventory ("MOCI", Hodgson and Rachman 1977), Mill Hill Vocabulary Scale Synonym Selection Test ("MH", Raven 1965) and Sandler and Hazari's obsessional personality and symptoms scale ("S+H", Sandler and Hazari 1960). For all of these measures, the means and standard deviations of the groups are given in Appendix A (see tables A.1-A.4); analyses of the results on these measures are given in the same Appendix (see tables A.5.1-A.6.2).

5.5.2.3 Procedure

The procedures used were the same as those described in Persons and Foa (1984). Before being presented with the packs of cards, subjects were read the following instructions: "I am going to give you five packs of cards to sort out. The first is a practise pack. Each of the cards will have on it the name of an object or a description of a situation, and each pack is to be sorted in terms of a particular idea or concept, which will be identified on the first card. For example, all of the objects named on the cards of this first practice pack are to be sorted in terms of the idea of "weight" and you will see the word "weight" on the first card. Please sort the objects named on the remaining cards in this pack into groups such that those objects which seem to you to weigh the same as one another are placed together in the same group. A group may contain as many items as you wish, or as few as just one or two items. I want to see how long it takes you to complete the sorting of each pack of cards and so I shall be timing you, but take as long as you like. Any questions?"

Subjects were tested individually by the same experimenter (ICJ) during an

experimental session lasting about two and a half hours. During this session, they were also required to complete the control measures, as well as the other two tasks described below (see section 5.9 and Chapter 6). The subject also completed a consent form (see Appendix B).

5.5.3 Results

The means and standard deviations of the groups on all of the "time taken" and "number of category" measures on Persons and Foa's task are shown on tables 5.A-5.C.2. A one way analysis of variance (SPSS:PC+ v.3.1 [1988]) of the number of categories used to sort all of the packs of cards combined showed no significant overall effect for group (F[5,47]=0.69, p=0.63). There was also no overall effect for group as regards the number of categories used to sort either the "feared" cards (that is, the "contaminated or dirty" and "seriousness of mistakes" packs combined) (F(5,47)=0.72, p=0.61) or the "non feared" cards (that is, the "size" and "temperature" packs combined) (F(5,47)=0.66, p=0.65). The time taken to sort the packs also showed no overall effect for group for all the packs of cards combined (F[5,47]=0.97, p=0.45). There was similarly no overall effect for group with either the "feared" (F(5,47)=0.70, p=0.63) or the "non feared" (F(5,47)=1.06, p=0.40) packs. There were also no overall effects for group as to the number of cards used or the time taken to sort any of the four packs of cards when these were analysed individually (number of categories: size [F{5,47}=0.53, p=0.75], contamination [F{5,47}=0.89, p=0.50], temperature [F{5,47}=1.15, p=0.35], seriousness of mistakes [F{5,47}=0.43, p=0.83]; time taken: size [F{5,47}=1.40, p=0.24], contamination [F{5,47}=0.81, p=0.55], temperature [F{4,45}=0.68, p=0.61], seriousness of mistakes [F{4,45}=0.64, p=0.64]). Unequal variances were observed across the groups as regards the time taken to sort the "temperature" cards (Cochran's C=0.40, p=0.03), and the "seriousness of mistakes" cards (Cochran's C=0.41, p=0.02). Analysis of variance is, however, robust with respect to its assumption of equal variances, provided that the groups with which it is dealing are approximately equal in size (Erickson and Nosanchuk, 1977, p184). Group 6 was therefore excluded from the analyses of the results for these packs.

Contrasts were specified for all of these variables, the results of which are displayed in tables 5.D-5.I.2. Due to the unequal variances observed across the groups as regards the "time taken" measure on the "temperature" and "seriousness of mistakes" packs, group 6 was once again excluded from these contrasts for the "time taken" scores for these packs, and the results reported based upon separate variance estimates. In terms of both the number of categories used and the time taken to sort any of the packs, there were no significant differences between the OCD patients (groups 1, 2 + 6) and the non OCD controls (groups 4 and 5), between the cleaners (group 1) and the checkers (group 2) or between the cleaners

(group 1) and the non OCD controls (groups 4 + 5). The checkers (group 2) used a significantly smaller number of categories than the non OCD controls to sort the "temperature" cards, and took significantly more time than the non OCD controls to sort the size cards. Further details relevant to the latter finding will be offered below.

There were no significant differences on any measure between the ex OCD patients (group 3) and either the OCD patients (groups 1, 2 + 6), the cleaners (group 1), the checkers (group 2) or the non OCD controls (groups 4 + 5).

The means for the "time taken" scores suggest the groups of OCD patients and ex patients - especially the checkers - to be somewhat slower than the non OCD controls. The data were therefore examined more closely to see if there were some OCD patients (or ex patients) who were clearly distinguishable from non OCD controls in terms of the time they required to sort the packs. There was a tendency for some OCD patients (and an ex patient) to take a good deal more time to sort the packs than any of the non OCD controls. Tables 5.J and 5.K show the time taken by the four slowest subjects in each of the groups 1-3, cleaners, checkers and ex OCD patients) for each of the four packs; the time taken by the two slowest subjects in each of the groups 4 (psychiatric controls) and 5 (normal controls) for each of the packs is also shown. Each subject's number is given in brackets beside his or her time. An asterisk marks all of those times in groups 1-3 which are slower by at least 50 seconds than the time taken for the slowest subject in either of the non OCD control groups on the pack in question. It can be seen that two OCD subjects (11+15) were on two of the packs slower by more than 50 seconds than any non OCD control subject, and that two OCD subjects (17+19) and one ex OCD control (25) were on three of the packs slower than any non OCD control by more (in all three of these cases, often a great deal more) than 50 seconds than any non OCD control subject.

All four of the OCD patients with slower "time taken" scores were checkers; the ex OCD patient with such scores would have been classified as such when ill. None of these five subjects had ever reported cleaning difficulties in addition to their checking behaviour. These five subjects differed widely from one another in age; three of them were female, two male. The four subjects from group 2 differed widely from one another in terms of their BDI and Mill Hill scores. Most of these four tended to have moderate to high scores on the checking and doubting subscales of the MOCI, and moderate to high total scores on this scale, as compared with other OCD patients in the present sample. The fifth subject, the ex OCD patient, had by far the highest score in his group on the BDI questionnaire, and the group's joint highest checking subscale and total score on the MOCI (although his BDI and MOCI

scores were only moderately high as compared to the groups of OCD patients). His Mill Hill score was among the highest in the whole sample. Greater "time taken" scores did not tend to occur more on the "feared" than the "non feared" cards; there was some tendency for these greater scores to be related to the order in which the packs were presented - the first two packs, "size" and "dirty or contaminated", tending to produce larger scores among the OCD patients than the last two packs did.

There were no patients among the cleaners whose results were as distinguishable from those of the non OCD controls as were those of these five subjects. Some of the group 1 patients did tend, however, to take longer to sort the packs of cards than the non OCD subjects, and as a result only 3 of the 5 subjects discussed above (17, 19 + 25) took more than fifty seconds than the slowest patient from group 1 on two of the packs, none of these three doing so on a third pack.

The three OCD patients in group 6 had scores on the "time taken" measure which were indistinguishable from the non OCD controls on all packs of cards.

The two patients from group 2 who reported cleaning as well as checking difficulties were excluded in further analyses, thus forming a group of eight "non cleaning checkers". All of the contrasts previously specified for group 2 were now conducted with these "non cleaning checkers". Their "time taken" scores on the first two packs were significantly slower than those of the non OCD controls (groups 4 + 5) ("size" pack: t=2.52 with 45 df, p=0.02, "contaminated or dirty" pack: t=2.52, with 45 df, p=0.02). None of the other contrasts involving these patients were significant.

Table 5.L shows the group means and standard deviations for the differences between the "feared" and "non feared" cards in terms of the time taken and number of categories used. Paired t-tests showed that all subjects (groups 1-6 combined) used significantly fewer categories to sort the feared cards than they did to sort the non feared cards and took significantly more time to sort the feared cards than they did to sort the non feared cards (see table 5.M). The difference between the number of categories used to sort the feared and non feared cards was also significant for all the OCD patients (groups 1 , 2 + 6 combined), for the cleaners (group 1), for the checkers (group 2), for the ex OCD patients (group 3) and for the non OCD controls (group 4 + 5) (also see table 5.M). The difference between the time taken to sort the feared and non feared cards, however, failed to reach significance for all the OCD patients (groups 1, 2 + 6), for the cleaners (group 1), for the checkers (group 2), for the ex OCD patients (group 3) and for the non OCD controls (group 4 + 5) (see table 5.N). Paired t-tests also showed that the situation as regards the 8 "non cleaning checkers" (see above) was

similar - the difference between the number of categories they used to sort the feared and non feared cards was significant (difference mean=-8.63 SE=1.55 t=-5.58 with 7 df, p<0.01); the difference between the time they took to sort the "feared" and the "non feared" cards was not (difference mean=69.13 SE=103.96 t=0.66 with 7 df, p=0.53).

It was finally considered whether any of these foregoing differences were themselves significantly different - whether, that is, there were any significant differences between any of the differences between the number of categories used, and/or the time taken by, the various groups of subjects to sort the "feared" and the "non feared" cards. This was examined by computing for each subject two "difference scores". One of these scores consisted of the "number of categories" result for each subject on the feared cards minus his or her "number of categories" result for the non feared cards. The other difference score consisted of the time taken by each subject on the feared cards minus the time taken by him or her on the non feared cards.

(In Persons and Foa's study, the same question was investigated for both the "number of categories" and "time taken" measures by means of two way "Group" x "Type of Task" ANOVAs, with repeated measures on the "Type of Task" variable - the main effect for "Type of Task" being modified by a significant "Group" x "Type of Task" interaction on the "time taken" ANOVA. The analyses of "difference scores" in the present investigation amounts to an alternative means to the same end.)

A one way analysis of variance of the "difference scores" showed no significant overall effect for group (time taken "difference score": F[4,45]=0.01 [p>0.99], number of categories "difference score": F[4,45]=0.82 [p=0.52]). Unequal variances were observed across the groups as regards both of the "differences scores" (time taken "difference score": Cochran's C=0.49 p=0.01, number of categories "difference score": Cochran's C=0.40, p=0.03). As with the earlier results with unequal variances, therefore, Group 6 was excluded from the analyses of these scores, and the contrasts examined were based upon separate variance estimates. The same contrasts as were used earlier were also specified for the "difference scores"; two of these reached significance - the cleaners had significantly lower "number of categories" "difference scores" than both the non OCD controls (t=2.39 with 24.2 df, p=0.03) and the 8 "non cleaning checkers" from group 2 (t=2.26 with 12.9 df, p=0.04).

Table 5.A: The means and standard deviations of the time taken (in seconds) and the numbers of categories used by the groups on Persons and Foa's task to sort all the packs of cards combined

	All OCD (N=23)	All OCD	Checkers (N=10)	Checkers	Cleaners (N=10)	Cleaners	Ex-OCD pts (N=10)	Ex-OCD pts	Non OCD ctls (N=20)	Non OCD ctls
	M	SD	M	SD	M	SD	M	SD	M	SD
Number of cat.	35.87	10.40	33.00	8.03	39.80	12.92	38.30	14.16	39.00	11.25
Time	815.52	416.60	873.40	468.04	810.90	433.90	836.40	442.81	633.30	232.68

Note: "cat." = categories, "ctls" = controls

Table 5.B: The means and standard deviations of the time taken (in seconds) and the numbers of categories used by the groups on Persons and Foa's task to sort the "feared" and "non feared" cards

	All OCD (N=23)	All OCD	Checkers (N=10)	Checkers	Cleaners (N=10)	Cleaners	Ex OCD (N=10)	Ex OCD	Non OCD controls (N=20)	Non OCD controls
	M	SD	M	SD	M	SD	M	SD	M	SD
Fear cards - no. of cat.	14.91	5.85	12.60	4.0	17.7	7.2	15.6	9.7	15.45	6.46
Non fear cards - no. of cat.	20.96	5.40	20.4	5.3	22.1	6.1	22.7	6.3	23.55	5.81
Fear cards - time	429.48	256.45	461.40	309.45	431.80	239.91	447.40	274.34	343.35	142.73
Non fear cards - time	386.04	197.99	412.00	220.00	379.10	212.58	389.00	180.97	289.95	115.27

Note: "cat." = categories

Table 5.C.1: The means and standard deviations of the time taken (in seconds) and the number of categories used by the groups on Persons and Foa's task to sort the "dirty or contaminated" and "seriousness of mistakes" cards

	All OCD (N=23)	All OCD	Checkers (N=10)	Checkers	Cleaners (N=10)	Cleaners	Ex OCD (N=10)	Ex OCD	Non OCD Controls (N=20)	Non OCD Controls
	M	SD	M	SD	M	SD	M	SD	M	SD
Dirty or cont. - no. of cat.	8.61	3.19	7.2	2.14	10.30	3.80	8.7	5.17	8.9	3.88
Ser. of Mis. - no. of cat.	6.30	2.96	5.40	2.27	7.40	3.72	6.90	4.84	6.55	3.24
Dirty or cont. - Time	201.87	117.60	227.80	151.50	186.10	90.30	223.20	143.36	159.20	57.47
Ser. of Mis. - Time	227.61	165.21	233.60	201.40	245.70	153.40	224.20	136.58	184.15	91.83

Note: "cont." = contaminated, "cat." = number of categories, "ser. of mis." = seriousness of mistakes

Table 5.C.2: The means and standard deviations of the time taken (in seconds) and the number of categories used by the groups on Persons and Foa's task to sort the "size" and "temperature" cards

	All OCD (N=23)	All OCD	Checkers (N=10)	Checkers	Cleaners (N=10)	Cleaners	Ex OCD (N=10)	Ex OCD	Non OCD controls (N=23)	Non OCD controls
	M	SD	M	SD	M	SD	M	SD	M	SD
Size - no. of cat.	12.17	3.23	12.6	2.80	12.4	3.86	13.5	4.77	13.55	4.14
Temp - no. of cat.	8.78	2.76	7.80	2.86	9.70	7.9	9.2	2.62	10.00	2.45
Size - time	218.74	121.16	232.80	122.40	213.60	142.64	223.50	118.76	145.35	61.63
Temp - time	167.30	89.77	179.20	113.50	165.50	78.93	165.50	70.20	144.60	63.87

Note: "cat." = categories, "temp" = temperature

Table 5.D: Contrast results for the time taken (in seconds) and the number of categories used to sort all the packs of cards combined on Persons and Foa's task - OCD groups and controls

	Number of categories to sort cards	Time taken to sort cards
All OCD groups vs non OCD controls	t=-1.04 (p=0.30)	t=1.16 (p=0.25)
Checkers vs cleaners	t=1.32 (p=0.19)	t=-0.38 (p=0.71)
Checkers vs non OCD controls	t=-1.34 (p=0.19)	t=1.68 (p=0.10)
Cleaners vs non OCD controls	t=0.18 (p=0.86)	t=1.24 (p=0.22)

Note: All t values quoted have 47 degrees freedom

Table 5.E: Contrast results for the time taken (in seconds) and number of categories used to sort the "feared" and "non feared" cards on Persons and Foa's task - OCD groups and controls

	Number of Categories to sort Feared cards	Number of Categories to sort non Feared cards	Time to sort Feared cards	Time to sort non Feared cards
All OCD groups vs non OCD controls	t = -0.40 (p= 0.69)	t = -1.58 (p= 0.12)	t = 0.79 (p= 0.43)	t = 1.44 (p= 0.16)
Checkers vs cleaners	t = 1.66 (p= 0.10)	t = 0.65 (p= 0.52)	t = -0.29 (p= 0.77)	t = -0.43 (p=0.67)
Checkers vs non OCD controls	t = -1.07 (p= 0.29)	t = -1.39 (p= 0.17)	t = 1.34 (p= 0.19)	t = 1.84 (p= 0.07)
Cleaners vs non OCD controls	t = 0.84 (p= 0.40)	t = -0.64 (p= 0.52)	t = 1.0 (p= 0.32)	t = 1.35 (p= 0.18)

All t values quoted have 47 degrees of freedom.

Table 5.F.1: Contrast results for the time taken (in seconds) and number of categories used to sort the "contaminated or dirty" and "seriousness of mistakes" cards on the Persons and Foa's task - OCD groups and controls

	Number of categories to sort "contaminated or dirty" cards	Number of categories to sort "seriousness of mistakes" cards	Time to sort "contaminated or dirty" cards	Time to sort the "seriousness of mistakes" cards
All OCD groups vs non OCD controls	t=-0.41 (p=0.69)	t=-0.34 (p=0.74)	t=-0.98 (p=0.33)	t=1.24 with 24.2 df (p=0.23)
Checkers vs cleaners	t=1.82 (p=0.08)	t=1.27 (p=0.21)	t=-0.87 (p=0.39)	t=0.15 with 16.8 df (p=0.88)
Checkers vs non OCD controls	t=-1.15 (p=0.26)	t=-0.85 (p=0.40)	t=-1.66 (p=0.11)	t=0.74 with 10.8 df (p=0.47)
Cleaners vs non OCD controls	t=0.95 (p=0.35)	t=0.63 (p=0.54)	t=0.95 (p=0.35)	t=1.17 with 12 df (p=0.26)

All t values quoted have 47 degrees of freedom unless otherwise stated

Table 5.F.2: Contrast results for the time taken (in seconds) and the number of categories used to sort the "size" and "temperature" cards on Persons and Foa's task - OCD groups and controls

	No. of categories to sort "size" cards	No. of categories to sort "temperature" cards	Time to sort "size" cards	Time to sort "temperature" cards
All OCD groups vs non OCD controls	t=-1.44 with 47 df, p=0.16	t=-1.35 with 47 df, p=0.18	t=1.92 with 47 df, p=0.06	t=1.07 with 27.5 df, p=0.29
Checkers vs cleaners	t=-0.11 with 47 df, p=0.91	t=1.63 with 47 df, p=0.11	t=-0.41 with 47 df, p=0.68	t=-0.31 with 16.1 df, p=0.76
Checkers vs non OCD controls	t=-0.62 with 47 df, p=0.54	t=-2.18 with 47 df, p=0.04	t=2.16 with 47 df, p=0.04	t=0.90 with 11.8 df, p=0.39
Cleaners vs non OCD controls	t=-0.75 with 47 df, p=0.46	t=-0.30 with 47 df, p=0.77	t=1.68 with 47 df, p=0.10	t=0.73 with 14.7 df, p=0.48

Table 5.G: Contrast results for the time taken (in seconds) and the number of categories used to sort all of the packs of cards on Persons and Foa's task - ex OCD patients, OCD groups and controls

	Number of categories to sort cards	Time taken to sort cards
Ex OCD patients vs all OCD patients	t=-0.71 (p=0.48)	t=-0.42 (p=0.67)
Ex OCD patients vs non OCD controls	t=-0.16 (p=0.88)	t=1.42 (p=0.16)
Ex OCD patients vs checkers	t=-1.03 (p=0.31)	t=0.22 (p=0.82)
Ex OCD patients vs cleaners	t=1.32 (p=0.19)	t=-0.38 (p=0.71)

All t values quoted have 47 degrees of freedom

Table 5.H: Contrast results for the time taken (in seconds) and number of categories used to sort the "feared" and the "non feared" cards on Persons and Foa's task - ex OCD patients, OCD groups and controls

	Number of categories to sort Feared cards	Number of categories to sort non Feared cards	Time to sort Feared cards	Time to sort non Feared cards
Ex OCD patients vs all OCD patients	t = -0.38 (p= 0.70)	t = -0.95 (p= 0.35)	t = -0.49 (p= 0.63)	t = -0.26 (p= 0.80)
Ex OCD patients vs non OCD controls	t = 0.06 (p= 0.96)	t = -0.38 (p= 0.71)	t = 1.18 (p= 0.24)	t = 1.50 (p= 0.14)
Ex OCD patients vs checkers	t = -0.97 (p= 0.34)	t = -0.88 (p= 0.38)	t = 0.14 (p= 0.89)	t = 0.30 (p= 0.77)
Ex OCD patients vs cleaners	t = 0.68 (p= 0.50)	t = -0.23 (p= 0.82)	t = -0.15 (p= 0.88)	t = -0.13 (p= 0.90)

All t values quoted have 47 degrees of freedom

Table 5.I.1: Contrast results for the time taken (in seconds) and number of categories used to sort the "contaminated or dirty" and "seriousness of mistakes" cards on Persons and Foa's task - ex OCD patients, OCD groups and controls

	Number of categories to sort "contaminated or dirty" cards	Number of categories to sort "seriousness of mistakes" cards	Time to sort "contaminated or dirty" cards	Time to sort the "seriousness of mistakes" cards
Ex OCD patients vs All OCD groups	t=-0.03 (p=0.97)	t=-0.53 (p=0.60)	t=-0.68 (p=0.50)	t=0.26 with 22.3 df (p=0.80)
Ex OCD patients vs checkers	t=-0.14 (p=0.89)	t=-0.96 (p=0.34)	t=1.54 (p=0.13)	t= 0.12 with 15.8 df (p=0.90)
Ex OCD patients vs cleaners	t=0.94 (p=0.35)	t=0.32 (p=0.75)	t=-0.78 (p=0.44)	t=0.33 with 17.8 df (p=0.75)
Ex OCD patients vs non OCD controls	t=-0.88 (p=0.38)	t=0.26 (p=0.80)	t=0.10 (p=0.92)	t=0.84 with 12.8 df (p=0.42)

All t values quoted have 47 degrees of freedom unless otherwise stated

Table 5.I.2: Contrast results for the time taken (in seconds) and number of categories used to sort the "size" and "temperature" cards on Persons and Foa's task - ex OCD patients, OCD groups and controls

	Number of categories to sort "size" cards	Number of categories to sort "temperature" cards	Time to sort "size" cards	Time to sort "temperature" cards
Ex OCD patients vs All OCD groups	t=-1.16 with 47 df, p=0.25	t=-0.35 with 47 df, p=0.73	t=-0.28 with 47 df, p=0.78	t=0.22 with 22.9 df, p=0.83
Ex OCD patients vs checkers	t=-0.51 with 47 df, p=0.62	t=-1.20 with 47 df, p=0.24	t=0.20 with 47 df, p=0.84	t=0.33 with 15.00 df, p=0.75
Ex OCD patients vs cleaners	t=-0.62 with 47 df, p=0.54	t=0.43 with 47 df, p=0.67	t=-0.21 with 47 df, p=0.83	t=0.00 with 17.80 df, p>0.99
Ex OCD patients vs non OCD controls	t=-0.03 with 47 df, p=0.97	t=-0.79 with 47 df, p=0.43	t=1.93 with 47 df, p=0.06	t=0.80 with 16.1 df, p=0.44

Table 5.J: Slowest scores (in seconds) in each of the groups 1-5 on Persons and Foa's task to sort the "size" and "contaminated or dirty" cards

Pack	Size	Size	Size	Size	Contamin-ated or dirty	Contamin-ated or dirty	Contamin-ated or dirty	Contamin-ated or dirty
Order	**1st**	**2nd**	**3rd**	**4th**	**1st**	**2nd**	**3rd**	**4th**
Cleaners	554* (8)	318 (4)	283 (5)	225 (7)	371* (4)	263 (5)	245 (8)	221 (7)
Checkers	415* (11)	410* (19)	358* (15)	255 (12)	467* (17)	462* (19)	346* (11)	228 (20)
Ex OCD pts	463* (25)	366* (28)	261 (23)	243 (29)	541* (25)	323 (23)	309 (28)	221 (27)
Psy. ctls	299 (36)	192 (40)			286 (39)	217 (31)		
Norm. ctls	304 (43)	162 (49)			250 (49)	237 (43)		

Note: "pts" = patients, "psy." = psychiatric, "norm" = normal

Table 5.K: Slowest scores (in seconds) in each of the groups 1-5 on Persons and Foa's task to sort the "temperature" and "seriousness of mistakes" cards

Pack	Temperature	Temperature	Temperature	Temperature	Ser. of Mistakes	Ser. of Mistakes	Ser.of Mistakes	Ser. of Mistakes
Order	**1st**	**2nd**	**3rd**	**4th**	**1st**	**2nd**	**3rd**	**4th**
Cleaners	302 (4)	276 (8)	200 (5)	194 (7)	475 (4)	432 (5)	388 (8)	382 (7)
Checkers	352* (19)	339* (15)	310* (17)	198 (11)	774* (17)	290 (12)	282 (15)	183 (11)
Ex OCD pts	270 (28)	245 (25)	202 (29)	197 (24)	541* (25)	315 (23)	291 (24)	248 (28)
Psy ctls	262 (36)	256 (39)			475 (39)	266 (34)		
Norm ctls	235 (43)	213 (44)			250 (44)	230 (43)		

Note: "ser." = seriousness, "pts" = patients, "psy" = psychiatric, "norm" = normal

Table 5.L: The means and standard deviations of differences between time taken (in seconds) and the number of categories used by the groups on Persons and Foa's task to sort the "feared" and "non feared" packs of cards

	All OCD (N=23)	All OCD	Checkers (N=10)	Checkers	Cleaners (N=10)	Cleaners	Ex OCD (N=10)	Ex OCD	Non OCD ctls (N=20)	Non OCD ctls
	M	**SD**	**M**	**SD**	**M**	**SD**	**M**	**SD**	**M**	**SD**
Diffs. in no. of cat.*	-6.04	4.31	-7.80	4.96	-4.40	3.34	-7.10	8.12	-8.10	4.94
Diffs. in time taken**	43.43	190.76	49.40	263.18	52.70	131.25	58.40	141.22	53.40	114.80

Note: "Diffs" = differences, "cat." = category

* Calculated as number of categories to sort feared cards minus number of categories to sort non feared cards
** Calculated as the time taken to sort the feared cards minus the time taken to sort the non feared cards

Table 5.M: Results of paired t-tests on the differences in number of categories used by the groups on Persons and Foa's task to sort the "feared" and the "non-feared" cards

	Difference Mean	Standard Error	t value	DF	2 Tail probability
All subjects	-7.02	0.74	-9.44	52	<0.01
All OCD groups	-6.04	0.90	-6.72	22	<0.01
Cleaners	-4.40	1.06	-4.17	9	<0.01
Checkers	-7.80	1.57	-4.97	9	<0.01
Ex OCD patients	-7.10	2.59	-2.74	9	0.02
Non OCD controls	-8.10	1.11	-7.33	19	<0.01

Table 5.N: Results of paired t-tests on the differences in time taken (in seconds) by the groups on Persons and Foa's task to sort the "feared" and the "non feared" cards

	Difference Mean	Standard error	t value	DF	2 Tail probability.
All subjects	50.02	21.15	2.37	52	0.02
All OCD groups	43.43	39.78	1.09	22	0.29
Cleaners	52.70	131.25	1.27	9	0.24
Checkers	49.40	83.22	0.59	9	0.57
Ex OCD patients	58.40	44.66	1.31	9	0.22
Non OCD controls	53.40	25.67	2.08	19	0.05

5.6 Discussion

Answers can now be provided to the four questions raised earlier. Taking each of these in turn, the present study found that:

(i) The OCD patients (groups 1, 2 + 6 combined) were not distinguishable on Persons and Foa's task from the non OCD controls (groups 4 + 5 combined) either in terms of the number of categories they used to sort the packs of cards or the time they took to do so.

(ii) The OCD patients whose major difficulties involved checking behaviour (group 2) used a significantly smaller number of categories to sort the "temperature" pack, and took significantly more time to sort the "size" pack, than did the non OCD controls. They were as a group otherwise indistinguishable on this task from the non OCD controls (groups 4 + 5 combined) and from the OCD patients whose major difficulties involved only cleaning (group 1) both in terms of the number of categories they used and the time they took to sort the packs of cards. A close inspection of the data revealed a tendency for some of the patients in group 2 to take considerably more time than any of the non OCD controls to sort two or three of the packs of cards. Because of these patients, the "time taken" scores of the 8 "non cleaning checkers" from group 2 were significantly slower than those of the non OCD controls on two of the packs. These 8 patients were as a group not significantly different from the cleaners (group 1) either in terms of the number of categories they used or the time they took to sort the packs of cards. The OCD patients whose major difficulties involved only cleaning (group 1) were indistinguishable from the non OCD controls both in terms of the number of categories they used and the time they took to sort the packs of cards.

(iii) The ex OCD patients (group 3) were indistinguishable on Persons and Foa's task from the OCD patients (groups 1, 2 + 6 combined), from the OCD patients whose major difficulties involved checking behaviour (group 2) and from OCD patients whose major difficulties involved only cleaning (group 1) both in terms of the number of categories they used and the time they took to sort out the packs of cards. One ex OCD patient, a former checker, tended to take much more time than any non OCD control, or any OCD patient from group 1, to sort some of the packs.

(iv) All subjects (groups 1-6 combined) required significantly fewer categories and significantly more time to sort the "feared" packs than they did to sort the "non feared" packs. The former difference was also significant for all of the individual groups and combinations of groups examined in the contrasts specified; the latter difference was not significant for any of these individual groups or combinations of individual groups. There were, apart from two exceptions, no significant differences between the "difference scores" of the groups.

Taking all of these results together, then, they provide little evidence in favour of the groups of OCD patients examined here having a specific or general cognitive style which distinguishes them from non OCD controls, this being so whether these groups are considered separately or combined. This is counter to Reed's predictions and to those of Persons and Foa's "complex concepts" hypothesis. The results also suggest, once again against Reed's account, that the ex OCD patients are not distinguishable on Persons and Foa's task from the non OCD controls; they are also indistinguishable from the groups of OCD patients (again, whether these OCD groups are considered separately or together). Five cases, most of these coming from the group of OCD checkers, were clearly distinguishable from all the non OCD controls, from most of the cleaners and from most of the ex OCD patients, but were so only, as with Frost et al's "non clinical compulsives", in terms of the time they took, not the number of categories they used, to sort the packs of cards. Clearly, this might be regarded as providing some, albeit very limited, support for Reed's account and Persons and Foa's "complex concepts" hypothesis. But the theoretical objections to Reed's account as applied to at least many cases of checking difficulties discussed elsewhere (Chapter 4) encourage the search for alternative accounts of this finding. A single finding also confirmed Persons and Foa's "simple concepts" hypothesis, the checkers (group 2) using fewer categories than the non OCD controls did to sort the "temperature" pack. The isolation of this confirmation, as well as the theoretical difficulties for the "simple concepts" hypothesis considered above, mean that little importance should be attached to this finding.

Almost all of the "difference scores" were indistinguishable from one another and a distinction must in any case be drawn here between the present study and that of Persons and Foa. As regards that investigation, it might be argued that the difference between the "difference scores" of the groups was important because while Persons and Foa's "obsessive compulsives" differed significantly from controls in terms of the time they took to sort both the "feared" and the "non feared" cards, only the "obsessive compulsives" took longer to sort the feared cards than they took to sort the non feared cards. Establishing whether or not the "difference score" of the "obsessive compulsives" was significantly greater than that of the control group on the "time taken" measure could therefore be argued by Persons and Foa to address the question of whether the "obsessive compulsives" were exhibiting a "specific" effect over and above the "general" effect which had already been observed regarding their performance (see above). However, in the absence of any indication that OCD patients are distinguishable from controls in terms of the "number of categories" or "time taken" measures with any of the packs of cards (as for the most part with the results of the present study), the

"difference scores" can no longer play this role - there can clearly be no question of an analysis of "difference scores" establishing whether or not there is a specific effect in addition to a general one if it has already been established that neither type of effect is present.

The almost complete failure of the present study, especially in the case of the "number of categories" findings, to replicate either Persons and Foa's results, or those of Reed (1969[ii]) with an alternative "inductive classificatory" task, is intriguing. In the case of Reed's study this may be in part due to his using only patients with obsessional personality disorders - it has been noted elsewhere (chapter x) that one might expect such patients to be more readily distinguishable from controls on experimental tasks than OCD patients would be. But it might be argued that this is unlikely to be a complete explanation, as one would expect more OCD patients than non OCD controls to have obsessional personalities (Black 1974). If it is this personality profile which produced the results reported by Reed with his obsessionals, one would surely expect OCD patients to resemble Reed's subjects to at least some extent on the task. A possible reply to this point, however, is that it is less clear that the obsessional personality disorder, as opposed merely to the personality, should be much more common among OCD patients than controls. It should also be noted that one has no way of knowing for certain how the subjects in the present study would have performed on Reed's block sorting task, and it may be that there are important differences between this task and Persons and Foa's from the point of view of Reed's account.

The contrast of the present results with Persons and Foa's is perhaps therefore still more difficult to fathom. The mean scores on the MOCI of the three OCD groups in the present study (groups 1, 2 + 6) are similar to the mean MOCI score of 19.6 quoted by Persons and Foa as regards their "obsessive-compulsives". Furthermore, all of the OCD patients in the present study, and all but three of the ex OCD patients, had total MOCI scores greater than 10, the minimum for inclusion in Persons and Foa's group, so the exclusion from that group of subjects scoring lower than this could not explain the contrasting results of the two studies. Similarly the mean BDI scores of the three OCD groups in the present study were very like the mean score of 19.3 quoted by Persons and Foa for their "obsessive-compulsives".

It may be difficult, then, to explain the discrepancy between the present findings and Persons and Foa's. What is clear is that the present study stands as a more powerful test of both Reed's account and Persons and Foa's "complex concepts" hypothesis than Persons and Foa's study does - among other advantages for the present study, it has over three times as many experimental subjects as Persons and Foa's (nearly five times as many if one includes the ex OCD patients) and excludes the results from no OCD patient who was tested.

There was no relationship between the BDI scores of the various groups in the present study and the performance of these groups on Persons and Foa's task, contrary to Persons and Foa's own suggestion that the level of depression of their "obsessive-compulsives" might account for at least some of the differences between that group and the controls in their study. Thus, in the present investigation the non checking cleaners (group 1) had significantly higher BDI scores than non OCD controls (groups 4 + 5) and ex OCD patients (group 3), and yet were indistinguishable from these groups on the task. (Four of the five subjects who were slowest on Persons and Foa's task also had BDI scores which were much lower than the great majority of those of the much quicker subjects in group 1.)

The fact that the results of Frost et al's study were not fully replicated with the groups of OCD patients here raises the possibility that high MOCI scores among normal subjects are related to something other than non clinical levels of obsessive compulsive experience and behaviour, and that this (these) other thing(s) helped produce the effect reported by Frost et al. The present findings, of course, also raise doubts as to Frost et al's suggestion that they only failed to replicate Persons and Foa's "number of categories" result due to the use of a non clinical population in Frost et al's study.

It is worth considering here a few points regarding Persons and Foa's task itself. Against one of Persons and Foa's claims for it, one might question the extent to which the task could demonstrate whether the hypothesised cognitive style of OCD patients is specific to their symptom contents or is instead a general feature of their functioning. While the hypothesis that OCD patients have a distinctive cognitive style which is specific to their symptom content might indeed be supported by these patients differing from controls only on the "feared" cards, it could be argued that results such as those reported here (that is, OCD patients for the most part not differing from controls on either the "feared" or "non feared" cards) are also consistent with this hypothesis. Thus, according to this argument, whatever the nature of the items named on the cards, sorting them into groups remains a "neutral" task, one which does not require the patient directly to confront the situations which provoke his hypothesised specific cognitive style.

An alternative hypothesis, which flatly contradicts the foregoing suggestion, nonetheless similarly argues that Persons and Foa's task would be a poor measure of how specific or general the supposed cognitive style of OCD patients is. This alternative predicts that OCD patients should be distinguishable from non OCD controls on all of the packs of cards in Persons and Foa's task. Thus, it has already been argued elsewhere (see Chapter 4) that if the central problem for OCD patients involves such matters as a fear of being criticised

or blamed for making mistakes, this fear might be able to explain why the performance of these patients on experimental tasks differs from that of controls (it is being assumed for the sake of argument here that a case could be made out for this fear being present to a greater degree in OCD patients than controls and indeed, it seems likely that several of the accounts considered earlier [see Chapter 3] would be consistent with this). If such an explanation could be presented for OCD patients needing more time and/or categories to complete Persons and Foa's task, this would mean both that findings such as Persons and Foa's would not support Reed's thesis (see Chapter 4) and that the contrast Persons and Foa try to draw between their "feared" and "non feared" packs would be implausible - both types of pack would, on this account, be "feared" for OCD patients in being possible sources of error for them.

How might, then, one argue that results such as Persons and Foa's could be due to a greater concern on the part of OCD patients not to make errors? One such argument would be that those subjects who are most anxious not to make errors on this task will form more categories and take more time than others because they will try harder and thus consider for each item more than just its most obvious feature or features when trying to classify it. This, then, might produce the same performance as that observed among Person's and Foa's "obsessive-compulsives". In contrast, the subject who is not anxious about making mistakes, and who will thus not try very hard on this task, might approach it simply by considering, for each item, only those most obvious features for the purposes of classification. This might produce the same performance on the task as that observed among Persons and Foa's controls. (The same arguments could also be applied to Reed's [1969{ii}] block sorting task.) If Persons and Foa's findings (or Reed's block sorting task finding) were shown to be replicable, therefore, this complication would need to be tested. The findings of the present investigation, of course, suggest that for the most part no such effect could have been taking place among the OCD patients included.

Given that one of the "feared" packs contains items specifically concerned with cleaning and contamination, and the other items specifically concerned with the kinds of mistakes checkers are frequently worried about making, it was clearly appropriate to examine these two packs separately (as well as together) for the checking and non checking cleaning groups included in the present study (it is surely surprising that Frost et al do not report any such analyses for their cleaning and checking sub-groups). The sorting requirements involved in Persons and Foa's "seriousness of mistakes" pack, however, would appear not to be closely related to the doubts experienced by OCD checkers. Persons and Foa's task requires subjects to group the cards in this pack in terms of how serious a mistake is involved in the action

described on each. But a checker's doubts do not concern how serious a mistake it would be, for example, to leave a door unlocked or to cause a car accident. His doubts are rather to do with whether or not he has locked the door or been responsible for an accident; and such doubts are not modelled by the sorting of Persons and Foa's "seriousness of mistakes" pack. This pack might perhaps best be regarded, therefore, as a further "neutral" set of cards and the failure of the group of checking patients in the present study to be distinguishable from any other group in their performance on this pack might by the same token be argued to be compatible with the "specific" cognitive style hypothesis. In contrast, the sorting requirements involved in the "contaminated or dirty" pack do appear to model some of the doubts experienced by OCD patients with cleaning difficulties. Patients with such difficulties are frequently uncertain as to exactly how contaminated a given object or situation is, and are therefore also uncertain as to how contaminated that object or situation is relative to others.

Someone who wanted to argue that OCD patients have a specific or general cognitive style could offer some further reasons over and above those already considered as to why this style should in the present investigation not have produced the results predicted by Reed and Persons and Foa on the "number of categories used" and "time taken" measures on Persons and Foa's task. Thus, it might be argued that the number of categories a subject uses to sort Persons and Foa's packs of cards need not increase in proportion to the complexity of that subject's concepts. This is because the items named on these cards have what may be described as "indeterminate characteristics", and a subject would be expected to think of more of these characteristics the more complex his concepts are. For example, suppose a subject with a "complex concept" of contamination were asked to classify two items which a subject with a more "simple concept" would be likely to group separately, such as "a person who just had a shower" and "poorly dressed people". The subject with the more "complex concept" would, it might be argued, be more inclined than others to think of such complications as there being no means of telling whether either a person who has just had a shower or a poorly dressed person has an infectious disease. It might be reasonably argued by such a subject, furthermore, that this "indeterminate characteristic " - having an infectious disease - is of central importance to how one should classify the items in question. The uncertainty as to such characteristics might well lead the subject to classify these items together (this appears to be as legitimate as classifying them separately on the basis of this uncertainty), and the subject's "complex concept" of contamination will thus have lead him to form only one category where a subject with a more "simple concept" would have formed two. (This difficulty does not arise in the case of Reed's block sorting task - the blocks have no "indeterminate characteristics" -

and will arise more in the case of Persons and Foa's "feared" packs of cards than in the case of their "non feared" packs, this latter point being closely related to the fact that subjects are required to make more estimates as to uncertain or possible outcomes in the case of Persons and Foa's "feared" than their "non feared" cards [see below].)

Consider now Persons and Foa's predictions for their "time taken" measure. It could be argued that an OCD patient's ideas as to, for example, the degree of contamination of items such as those included in the "contaminated or dirty" pack may well be rehearsed from everyday experience, and the patient may as a result not be slower at sorting these cards despite his using a "complex concept" of contamination (once again this difficulty does not arise as regards Reed's block sorting task, and might perhaps arise more in the case of at least some of the "feared" items in Persons and Foa's task than the "non feared" items).

The foregoing points, then, show how both the "number of categories" and "time taken" measures may fail as regards at least some of Persons and Foa's packs of cards to indicate the complexity of a subject's concepts. How might one, then, establish that a subject was operating with "complex concepts" if both of these points were to hold good? The best means of doing so would be to examine the criteria used by the subject to classify the test items and in particular by establishing whether or not these criteria involved remote or unlikely features of the items named. (Reed [1969{ii}] gives examples of such features being used by obsessionals in his block sorting task - see section 4.1.) It is the use of such criteria which determines whether or not a subject is operating with "complex concepts"; using more categories and taking more time on a task such as Persons and Foa's are perhaps likely, but certainly not inevitable, consequences of such criteria being employed.

An adjusted version of Reed's account termed elsewhere (section 4.4) the "revised thesis" suggests that the difficulties of OCD patients result from their manner of handling probabilistic information. What predictions would this account make as regards Persons and Foa's task? Some of the items which have to be sorted in this task do require probabilities to be estimated and this seems to be particularly so in the case of the items in the two feared packs, posing as they do such questions as "how contaminated is a poorly dressed person?" (see above) and "how serious a mistake is involved in leaving electrical appliances unplugged at night?". Probability estimates are less involved in the questions raised by the items in the "non feared" packs, for example "what is the size of a wristwatch?". The "revised thesis" will consequently predict, it might be argued, that OCD patients should take longer than controls to sort the "feared" packs of cards, due to their having to consider more of the possible consequences associated with the items included in these packs. The "revised thesis" would

perhaps appear, on this argument, not to make any predictions as to the number of categories subjects should use to sort the packs of cards and seems, on this same argument, also to predict that there should be no differences between OCD patients and controls on any measure as regards the "non feared" packs of cards. (The "revised thesis" might similarly be argued to predict that OCD patients will be indistinguishable from controls on any measure as regards the block sorting task described by Reed [1969{ii}]). A possible difficulty for these suggestions, however, is that on both Persons and Foa's task and Reed's block sorting task it might be argued that the "revised thesis" would predict the performance of OCD patients to be effected by their overestimating the likelihood of such outcomes as their being blamed for making mistakes on these tasks. The implications of this suggestion are similar to those of the hypothesis, already examined above, that obsessionals may be more concerned about making errors on these tasks, predicting that these patients might both use more categories and take more time to sort all of the packs of cards.

5.7 Summary

The present study provides little evidence in favour of Reed's account of OCD (or the "complex concepts" hypothesis). A number of drawbacks with Persons and Foa's task have been noted which, in conjunction with some general remarks regarding such experimental tasks which were offered elsewhere (see section 4.8), suggest that the task could by itself not provide very strong evidence in favour of Reed's account, whatever the findings reported. Conversely, some reasons were also noted as to why the task might fail to indicate the presence of the cognitive style hypothesised by Reed despite a subject's actually operating with such a style. The task also provides an at best highly uncertain test of the "revised thesis".

5.8 "Deductive classificatory" tasks - Reed's "Essentials Test"

Reed has attempted to support his account of obsessional personality disorder and OCD with data from experimental tasks other than his (1969[ii]) block sorting "inductive classificatory" test. His account also predicts, Reed suggests, that patients who suffer from obsessional disorders should tend to be more strict than other subjects in the definitions they offer for any given concept. To test this prediction, Reed (1969[i]) constructed a task in which subjects were given a list of concepts and required to select, from a set of alternatives for each, those features they regarded as being "essential" for these concepts - that is, features necessarily possessed by all normal or undamaged instances of each of these concepts. This task, Reed suggests, may be described as a "deductive classificatory" test, that is a task in which one is given some category or class, the objective being to determine what may be most

appropriately included in it (contrast the tasks described in section 5.2). Reed (1969[i], p781) called this task the "Essentials" test and his prediction (1969[i], p781) was that patients suffering from obsessional disorders should tend to select fewer features as essential to the concepts included (to "overdefine" more) than would non obsessional controls. (The experimental group in Reed's study, like that in his investigation with the block sorting task, had obsessional personality disorders.)

How, then, does Reed derive from his account his prediction for the performance of obsessional patients on his "Essentials" test? Reed (1969[i], 1985) himself does not discuss this in any detail, but he would presumably wish to argue that OCD patients will be more likely than non OCD controls to reject any feature as essential to any given concept because they will be more inclined to think of remote or unlikely counter-instances which show (or appear to them to show) the feature in question not to be essential to the concept. It is clearly possible to link this suggestion with a claim Reed makes elsewhere (1969[ii], 1985), that OCD patients should be more inclined to think of remote or unlikely reasons as to why no two or more given items should be placed together in the "inductive classificatory" task discussed in section 5.2.

To take an example from the "Essentials" test to illustrate this suggestion, the concept "table" is included in this test, and most normal subjects (Reed 1969[i], p782) treat as essential to it the feature "legs", which is included in the set of alternatives for this concept (see below). It is being suggested here that, according to Reed's hypothesis, obsessional patients should be more inclined than normal and other non obsessional controls to reject this feature, having thought of such cases as, for example, tables which stand on a solid base and tables which are suspended from ceilings. (Both of these are evidently valid counter-instances - despite points being scored on the test for including "legs" as an essential feature - but Reed would certainly not suggest that all or even most cases of what obsessionals would tend to take as counter-instances would actually be so - he explicitly states that their performance on the task should not be superior to that of matched controls [1969[i], p783].)

Reed (1969[i]) found that his obsessional subjects did indeed underline significantly fewer words and were also therefore significantly more likely to be classified as "over-definers" in terms of their "error types" (see below).

Reed suggests that the same general cognitive style should produce the patterns of performance which he predicts will be observed among obsessional patients on both "inductive" and "deductive" classificatory tasks. One is therefore entitled, in investigating his account, to look to studies which have used the former ("inductive") type of task for further

suggestions as to what may be observed as regards the performance of obsessional patients on the latter ("deductive") type of task, such as Reed's "Essentials" test. Some empirical investigations of OCD patients and "analogue" populations using "inductive classificatory" tasks suggest, as discussed in section 5.3.1, that the cognitive style described in Reed's account may be exhibited by some rather than all OCD patients, and in particular by those in whose difficulties checking behaviour predominates rather than those in whose difficulties only cleaning behaviour is observed (Frost et al 1988). Against this, theoretical considerations (see sections 4.1.2, 4.2 and 4.5) suggest that there is no good reason for expecting checkers to exhibit this style, and the study reported in section 5.5 above furthermore provided, in support of these considerations, little evidence of either checkers or cleaners exhibiting this style on an "inductive classificatory" task.

Another little researched question which is, as noted in section 5.5, of importance to Reed's (1985) account, is whether ex-OCD patients continue to exhibit, as that account predicts they should, the style it ascribes to them (see section 4.1). The study reported in section 5.5 again provided little reason for thinking that ex OCD patients do exhibit this cognitive style.

5.9 The present study

5.9.1 The questions investigated in the present study

The present study examined the following questions:

(i) Do OCD patients tend to underline fewer alternatives for the items on Reed's "Essentials" test (tend to "overdefine" more) than psychiatric and normal controls do?

(ii) Does the tendency to underline fewer alternatives (to "overdefine") on this test distinguish those OCD patients whose major difficulties involve checking behaviour not only from non OCD controls but also from those OCD patients whose major difficulties involve only cleaning? Or is it rather the patients whose major difficulties involve cleaning who are distinguishable in this way both from non OCD patients and from patients whose major difficulties involve only checking?

(iii) Are ex-OCD patients distinguishable from (some or all) OCD patients and/or non OCD controls in terms of the number of alternatives they underline on the test?

5.9.2 Method

5.9.2.1 Subjects

The subjects for this investigation are the same as those described for the investigation in section 5.5.2.1.

5.9.2.2 Materials and measures

The materials and measures used in administering the "Essentials" task for the present study were the same as those described in Reed (1969[i]). The instructions and questions for the task were as follows (in this example the answers deemed to be correct for items 1-14 have been underlined):

"In each of the following questions there is a word in capital letters followed by five other words. Your task is to decide in each case whether any of the five are essential to the first and underline them. In some cases none of the five may be essential to the first; in such cases underline the word "None" at the end of the line. Remember, only words which are essential count. There may be one, two, three, four or five such words on any line, or there may be none at all. Here is an example:

DOG. Head, collar, legs, kennel, tail. None.

Here the words "head", "legs" and "tail" have been underlined because they are all necessary parts of a normal dog. (Notice that in every case you must take the thing signified by the first word as being in a normal, undamaged state.) A collar and a kennel may both be desirable, but the dog is complete without them.

(i) TABLE. Cloth, vase, legs, drawer, top. None.

(ii) ROOM. Windows, door, walls, floor, furniture. None.

(iii) KNIFE. Metal, blade, sharp, handle, fork. None

(iv) ATHLETE. Medals, feet, neck, jersey, track. None

(v) BOOK. Shelf, cover, pictures, pages, print. None

(vi) PROCESSION. Band, flags, movement, police, people. None

(vii) ORCHESTRA. Instruments, musicians, hall, piano, music stands. None.

(viii) WATER. Tap, flowing, wetness, fresh, drinking. None

(ix) MOTOR CAR. Roads, wheels, garage, driver, engine. None

(x) HERO. Strong, soldierly, honest, patriotic, brave. None

(xi) SAINT. Hermit, bravery, poverty, dedication, priest. None

(xii) TREE. Roots, trunk, branches, leaves, seeds. None

(xiii) MOVEMENT. Time, smoothness, fat, weight, space. None

(xiv) SIN. Punishment, guilt, death, corruption, sadness. None"

Three scores were derived from the completed test forms:

(i) "Error types" score.

Test forms were examined to determine for each subject his or her predominant "error type". This was done by examining each answer which had been scored as incorrect to see

whether the error was due to too many or too few alternatives having been underlined. As in Reed's (1969[i]) investigation, there proved to be three major types of incorrect answers:

(1) errors where the right word or words had been underlined plus one or more other words. This was taken, as in Reed's study, as an "underdefining" type of error (too many alternatives selected).

(2) Those where, two or more words being required, less than the required number had been underlined, although those word or words which had been underlined were correct. This was taken as an "over-defining" type of error (too few alternatives selected).

(3) Those where only the word "none" had been underlined (this response was in all cases deemed incorrect). This was taken, as in Reed's (1969[i]) study, as an "over-defining" type of error - indeed as the limiting case of such errors - because no alternatives had been selected.

As in Reed's (1969[i]) study, other types of error were rare and have been discounted here. It was ascertained that their inclusion would have had no radical effect on the results. Errors having been analysed as above, each subject was then classified either as an "over-definer" or "under-definer" according to which type of error was most common in his or her responses. As noted above, Reed's account predicts that "overdefiners" should be more common among OCD patients than controls.

(ii) "Number of words" score

The second method of scoring, also chosen to demonstrate the patient's characteristic style of approach, was simply to total in each case the number of words underlined on the test (other than items where the word "none" had been underlined). As noted above, Reed's account predicts that obsessionals should tend to have lower scores on this measure than controls.

(iii) "Conventional" score.

This consisted of allotting one mark for each item where all and only the "correct" words were underlined. Items where too few or too many words were underlined were regarded as incorrect, even if one or more words had been underlined correctly. Thus, if only the word "top" was underlined on item (i) the response would receive no score. Similarly, if on item (viii) both "wetness" and "flowing" were underlined the item would receive no score. The maximum possible score was therefore 14. (Reed [1969{i}] found his obsessionals to be indistinguishable from control subjects on this measure.)

5.9.2.3 Procedure

Subjects were tested individually by the same experimenter (ICJ) during an

experimental session lasting about two and a half hours, during which they also completed the control measures (see Appendix A), a consent form (see Appendix B), Persons and Foa's task and the probabilistic inference task described below (Chapter 6). Although the test was presented in printed form, the examiner (ICJ) read through the instructions and each of the questions with subjects. No time limit was imposed.

5.9.3 Results

(i) "Error type" score

A Chi square test on the number of each "error type" ("underdefining" and "overdefining") found across the groups (see table 5.0) revealed no overall significant difference (see table 5.P). Certain groups were excluded from, and/or combined in, further chi square tests, allowing the various individual groups (and combinations of groups) which were examined in the contrasts specified for the results from the study using Persons and Foa's task (see section 5.5) also to be investigated here (see table 5.P). No significant differences emerged in any of these analyses. (A fisher's exact test was reported for the comparison of the cleaners [group 1] and checkers [group 2]; this too was not significant [2 tailed $p>0.99$].) Further analyses were conducted with the 8 "non checking cleaners" from group 2 (see sections 5.9.3 and 5.10). There were no significant differences between this group and the cleaners (group 1), the ex OCD patients (group 3) and the non OCD controls (groups 4 + 5).

(ii) "Number of words" score

Table 5.Q shows the group means and standard deviations for the "number of words" score. A one way analysis of variance of the number of words score showed there to be no overall effect for group ($F[5,45]=0.49$, $p=0.78$). The same eight contrasts as were specified as regards the measures in Persons and Foa's study (see section 5.5) were also examined for this score, none reaching significance (see table 5.R). Further contrasts were once again specified for the eight "non cleaning checkers" from group 2. These patients were once again indistinguishable from the cleaners (group 1), the ex OCD patients (group 3) and the non OCD controls (groups 4 + 5).

There was a tendency for some of the subjects in the psychiatric control group to underline a greater number of items (to "underdefine") more than subjects in the other groups. Thus, five patients in this group underlined a total of more than thirty six alternatives, whereas only one subject in each of the groups 1 and 2 (the OCD patients) and 3 (the ex OCD patients) underlined more than this number of alternatives.

(iii) Conventional Scores

As in Reed's study, the "conventional" scores were also examined. Table 5.Q shows

the "conventional" score group means and standard deviations. A one way analysis of variance of these scores showed no overall effect for group (F[5,45]=1.45,p=0.22). There was a significant difference between the "conventional" scores of the psychiatric and normal controls (t=-2.13 with 47 df, p=0.04), meaning that these two groups could not be combined to form a single non OCD control group, as they were for the analyses of the "number of words" scores. Contrasts were therefore specified instead for these two control groups separately, but were otherwise the same as those examined in the case of the "number of words" scores (see table 5.S). The psychiatric control group had significantly lower "conventional" scores than all the OCD groups (groups 1, 2 + 6 combined) and the checkers (group 2) and were also only just short of having significantly lower "conventional" scores than the cleaners (group 1) and the ex OCD patients (group 3). The contrasts shown in table 5.S otherwise revealed no significant differences. Further contrasts were again specified for the 8 "non cleaning checkers" from group 2. Their "conventional" scores were not significantly different from any of the other groups, although the psychiatric controls once again had scores which were almost significantly lower than those of these 8 patients (t=1.78 with 45 df,p=0.08).

Table 5.O: Number of "error types" in each group on the "Essentials" test

	"Underdefiners"	"Overdefiners"
Cleaners (N=10)	5	5
Checkers (N=10)	4	6
Ex OCD patients (N=10) *	5	4
Psychiatric controls (N=10)	7	3
Normal controls (N=10)	5	5
Non cleaning/checking patients (N=10)	2	1

* one subject in group had an equal number of "underdefining" and "overdefining" errors

Table 5.P: Chi square results for "error type" scores on the "Essentials" test - OCD patients, ex OCD patients and controls

	Chi Sq	DF	Significance
All groups	6.57	10	0.77
All OCD pts vs non OCD ctls	0.24	1	0.62
Checkers vs non OCD ctls	0.42	1	0.52
Cleaners vs non OCD ctls	0.02	1	0.90
Ex OCD pts vs all OCD pts	2.52	2	0.28
Ex OCD pts vs non OCD ctls	2.12	2	0.35
Ex OCD pts vs checkers	1.51	2	0.47
Ex OCD pts vs cleaners	1.11	2	0.47

Note: "ctls" = controls, "pts" = patients

Table 5.Q: The means and standard deviations for the "conventional" and "number of words" scores of the groups on the "Essentials" test

	No. of words	No. of words	"Conventional"	"Conventional"
	Mean	**SD**	**Mean**	**SD**
Cleaners (N=10)	28.80	5.87	6.8	2.7
Checkers (N=10)	29.00	5.87	6.9	1.73
Ex OCD patients (N=10)	27.40	5.34	6.8	2.49
Psy controls (N=10)	33.00	10.38	4.4	2.80
Normal controls (N=10)	30.10	12.20	7.0	3.77
Non ch/cl (N=3)	33.50	9.20	7.6	1.15

Note: "psy" = psychiatric, "non ch/cl" = non checking/cleaning patients

Table 5.R: Contrast results for "number of words" scores on the "Essentials" test - OCD patients, ex OCD patients and controls

All OCD pts vs non OCD ctls	Checkers vs cleaners	Checkers vs non OCD ctls	Cleaners vs non OCD ctls
t=-0.86, p=0.40	t=-0.05, p=0.96	t=-0.78, p=0.44	t=-0.84, p=0.41

Ex OCD pts vs all OCD pts	Ex OCD pts vs non OCD ctls	Ex OCD pts vs checkers	Ex OCD pts vs cleaners
t=0.52, p=0.61	t=-1.27, p=0.21	t=0.42, p=0.67	t=-0.37, p=0.71

All t statistics quoted have 47 df

Note: "ctls" = controls, "pts" = patients

Table 5.S: Contrast results for the "conventional" scores on the "Essentials" test - OCD patients, ex OCD patients and controls

All OCD pts vs psych ctls	All OCD pts vs normal ctls	Checkers vs psych ctls	Checkers vs normal ctls	Cleaners vs psych ctls	Cleaners vs normal ctls
t=2.5, p=0.02	t=0.11, p=0.91	t=1.97, p=0.06	t=-0.16, p=0.87	t=2.05, p=0.05	t=-0.08, p=0.94

Checkers vs non cleaners	Ex OCD pts vs all OCD pts	Ex OCD pts vs checkers	Ex OCD pts vs cleaners	Ex OCD pts vs psych ctls	Ex OCD pts vs normal ctls
t=-0.08, p=0.94	t=0.30, p=0.77	t=0.08, p=0.94	t=0.01, p>0.99	t=1.97, p=0.06	t=-0.16, p=0.87

All t values quoted have 47 df

Note: "pts" = patients, "psych ctls" = psychiatric controls, "normal ctls" = normal controls, "non ch cl"

5.10 Discussion

Answers can now be provided to the three question raised earlier. Taking each of these in turn, the present study found that:

(i) The OCD patients (groups 1, 2 + 6 combined) were indistinguishable from the non OCD controls (groups 4 + 5 combined) in terms both of their predominant "error type" and the number of words they underlined.

(ii) The OCD patients whose major difficulties involved checking behaviour (group 2) were as a group indistinguishable from the non OCD controls (groups 4 + 5 combined) and from the OCD patients whose major difficulties involved only cleaning (group 1) in terms both of their predominant "error type" and the number of words they underlined. The group of 8 "non cleaning checkers" in group 2 were similarly indistinguishable from the non OCD controls and from the OCD patients whose major difficulties involved only cleaning (group 1) in terms both of their predominant "error type" and the number of words they underlined. These OCD cleaners were also indistinguishable from the non OCD controls in terms both of their predominant "error type" and the number of words they underlined.

(iii) The ex OCD patients (group 3) were indistinguishable from all the OCD patients (groups 1, 2 + 6 combined), from the OCD patients whose major difficulties involved checking behaviour (group 2) (and the 8 "non cleaning checkers" in this group) and the OCD patients whose major difficulties involved only cleaning (group 1) both in terms of their predominant "error type" and the number of words they underlined.

Taking all of these results together, then, they suggest that the groups of OCD patients examined here do not have the cognitive style attributed to them by Reed's account; these results thus support the findings reported in section 5.5. The present results also suggest, similarly against Reed's position and with the investigation reported in section 5.5, that ex OCD patients do not have the cognitive style described by Reed's account.

Why is there a discrepancy between the present results and those reported by Reed (1969[i])? This may be in part due to his using only patients with obsessional <u>personality disorders</u> rather than OCD patients. As has already been argued elsewhere (see section 5.6), however, it is perhaps unlikely that this could provide a complete explanation of the discrepancy observed - one would expect more OCD patients than non OCD controls to have obsessional personalities (Black 1974). A possible reply to this point, as was also noted in section 5.6, is that it is at least less clear that one would expect obsessional personality <u>disorder</u> to be a great deal more common among OCD patients than controls.

There is no clear reason why the psychiatric control group contained more subjects

than any other group who underlined a very large number of alternatives on the "Essentials" test, and why therefore these controls had low "conventional" scores on the test. There was no evidence that the psychiatric controls had lower verbal intelligence than the other groups (see Appendix A). One should stress that the results of this group differentiated them as much from the normal controls as from the OCD groups and the group of ex OCD patients, and so this difference can not be a result of the absence of obsessive-compulsive symptomatology or personality traits among the psychiatric controls.

Consistent with Reed's (1969[i], p781) prediction, the conventional scores of the OCD patients in the present study were indistinguishable from those of the normal controls. It is of interest to note that elsewhere Reed (1985, p194) has suggested "anankastic over-structuring might well prove advantageous in...tasks requiring deductive reasoning" (original emphasis), and indeed Reed argues the superiority of obsessionals on such tasks to have been experimentally confirmed (Reed 1977[ii]). One might think, therefore, that Reed would also expect superior "conventional" scores on the present, "deductive classificatory" task. On this view, the "conventional" scores in the present study provide still further evidence against Reed's account.

It is worth considering here a few points regarding the "Essentials" test itself. Thus, against Reed's own interpretation of the results of his (1969[i]) study, might it not be that his obsessionals subjects produced the result he reports simply because they were concerned about being criticised for making errors on the task (that is, including the wrong alternatives as essential), as was similarly argued (see section 5.6) in the case of the results of studies using Reed's (1969) and Persons and Foa's (1984) "inductive classificatory" tasks? (It is again being assumed for the sake of argument here that a reasonable case could be made out for this concern about possible criticism being greater in the case of obsessionals than controls - also see section 5.6.) Against this suggestion, it might be objected that it would predict in the case of the "Essentials" test that obsessionals should be equally concerned about excluding the wrong items. On this objection, therefore, such a concern could not explain Reed's (1969[i]) results. But this objection may be countered. Other things being equal, to include an alternative as essential to a given concept is to make a bigger claim than that made by denying the alternative in question to be so. The latter claim merely states that one can present at least one instance of the given concept which does not possess the feature in question whereas the claim that a feature is essential to the concept amounts to the statement that there is not one instance of that concept which fails to exhibit it. This is a claim of which, other things being equal, it will be more difficult to be certain. It is reasonable to suggest, therefore, that a

concern not to make errors could produce results on this task like those of the obsessional subjects in Reed's (1969[i]) study - such a concern might indeed lead subjects to exclude as essential more items than those excluded by subjects who are less bothered by the possibility of making errors. Alternatively, and as with Persons and Foa's task (see section 5.6), one might suggest that a concern not to make errors could account for the performance of obsessionals in Reed's investigation by causing them, through trying harder, to think of more (genuine or erroneous) counter-instances to any supposedly essential feature. These possible alternative explanations would need to be checked, therefore, if findings such as Reed's with the "Essentials" test were to prove replicable. The present findings, of course, suggest no such effects to be taking place.

The "Essentials" test might be argued to be in some respects a better test of Reed's account than Persons and Foa's task. It had been argued above (see section 5.6) that Persons and Foa's task might produce "false negatives" as to the cognitive style postulated by Reed due to the "indeterminate" characteristics of some of the items included in Persons and Foa's task and the possibility that some of the classificatory demands of the task may be rehearsed from everyday life. It seems that neither difficulty arises in the case of the "Essentials" test.

An adjusted version of Reed's account, termed elsewhere (section 4.4) the "revised thesis", suggests that the difficulties of obsessionals stems from their style of dealing with probabilistic information. As the items included in the "Essentials" test do not involve the handling of such information, this task might be argued not to stand as a test of the "revised thesis" at all. Against this and again as with the results from "inductive cassificatory" tasks (see section 5.6), it might be suggested that the "revised thesis" would predict the performance of obsessionals to be effected by their overestimating the likelihood of such outcomes as their being blamed for making mistakes on the task. The implications of this suggestion are similar to those of the hypothesis, already examined above, that obsessionals may be more concerned about making errors on this task, predicting that these patients might both use more categories and take more time to sort all of the packs of cards.

5.11 Summary

The present study provides no evidence at all in favour of Reed's account of OCD. A number of points similar to those made in section 5.7 are also pertinent here. Thus, a few difficulties as regards the "Essentials" test itself have been noted in the foregoing which, in conjunction with some remarks regarding such experimental tasks in general which were offered elsewhere (section 4.8), suggest that the task could in any case by itself not provide very strong evidence in favour of Reed's account. It has also been suggested, however, that

the "Essentials" test is if anything a better test of Reed's thesis than Persons and Foa's task. Like that task, the "Essentials" test provides an at best highly uncertain test of the "revised thesis".

Chapter six: A probabilistic inference task

6.1 Introduction

As discussed elsewhere (see Chapter 4), Reed (1985, 1991) suggests that the thinking style detailed in his account makes it difficult for obsessionals to establish whether or not their actions have been properly completed; Reed argues that obsessionals compensate for this difficulty by "overstructuring" tasks and situations. Reed points out that this account is consistent with obsessionals having difficulties handling probabilistic information. Thus, he suggests (1991, p86) that the tendency of obsessionals to "overstructure" tasks and situations will lead them to have "an exaggerated need for exact information, and thus higher criteria than other people as to what constitutes a "satisfactory" level of proof"; they will require, Reed argues, "more and/or better evidence before they are prepared to make a decision".

An approach to OCD termed the "revised thesis" which can be derived from Reed's account has been presented elsewhere (see section 4.4); this thesis suggests that obsessionals are poor at distinguishing between the likely and unlikely possible outcomes of any given situation or action.

Both Reed's account and the "revised thesis" predict, then, that OCD patients should be more cautious than others when handling probabilistic information and should be so even when that information is unconnected to the contents of their symptoms (see sections 4.1 and 4.4). A task in which this prediction was examined would, therefore, test both Reed's account and the "revised thesis" (and could consequently in this respect be contrasted with both Persons and Foa's task and the "Essentials" test - see sections 5.6 and 5.10 above).

Such a task, devised by Phillips and Edwards (1966), in which subjects were required to use beads drawn from one of two possible jars as probabilistic information, was used by Volans (1974, 1976) in an investigation of OCD sufferers. Volans's study was inspired by her account of (some cases of) OCD, which is to a certain extent similar to the "revised thesis". She (1976, p303) suggested that "the behaviour of those obsessionals whose pathology includes excessive repetition" cannot be explained by learning theories (also see section 3.2), and she followed Beech (1971) in suggesting that "a decision theory model" might provide a more appropriate explanation of [such] behaviour" (1976, p305). Volans argued (p306) that it may be possible to relate the "real life disorder of decision-making of...[these] obsessional patients...to deficits in the processing of probabilistic information". On the basis of this, she predicted that on Phillips and Edwards's task (1) those OCD patients who report "an abnormally high degree of repetitive checking should request more probabilistic information before reaching a decision [as to which of two possible hypotheses is true]...than would either

phobic or normal subjects" (p306) and (2) that these OCD patients should similarly tend on this task to be more conservative than phobics and normal controls in their "interim judgments of hypothesis probability" (that is, to be more conservative than controls in their judgments of the relative likelihood of two possible hypotheses prior to deciding that one or other of them is true). Volans measured this latter variable in terms of the extent to which her subjects' scores deviated from what she describes as the "optimum Bayesian model" of decision making. (Phillips and Edwards's task has been extensively studied with normal subjects, who have been observed to make few errors but to tend towards conservatism in reaching a decision [Edwards 1982]).

Volans's results appear to provide some evidence in favour of both of her predictions regarding her OCD group. Thus, as regards her first prediction, she found that OCD patients (8 subjects all of whom reported a high degree of checking, as measured by a questionnaire devised by Volans herself) required more probabilistic information than non OCD controls (eight normals and eight phobics) before making a decision on Phillips and Edwards's task. Although this difference did not reach significance, Volans further reported that "neuroticism", as measured by the Eysenck Personality Inventory (EPI) (Eysenck and Eysenck 1964), was the best predictor of the amount of probabilistic information required before reaching a decision (being negatively correlated with this variable); Volans conducted an analysis of covariance with neuroticism partialled out, and found that "the three groups [did] differ significantly from one another in the predicted direction" in this analysis (p314).

As regards Volans's second prediction, she reports that the "deviation scores" of her OCD group (the extent to which their interim judgments of hypothesis probability differed from those of the "optimum Bayesian model") were significantly higher than those of her non OCD control groups. (This finding was reported without neuroticism being partialled out.)

Yet these results do not entirely confirm Volans's predictions. Thus, although Volans's (p306) first prediction was that her OCD group would request more probabilistic information before reaching a decision than would either phobic or normal subjects, her phobic subjects were in this respect if anything more like her OCD patients than her normals. When neuroticism was partialled out, the mean score for phobics on this measure was 26.56, while for her OCD patients and normals mean scores after partialling out were 38.46 and 11.1 respectively. Furthermore, it is not possible to determine from the analysis Volans reports (p313-4) whether the differences between her phobics and normal subjects, and/or that between her phobics and OCD patients, reached significance.

In the case of Volans's second prediction, she points out (p316) that as well as having

higher "deviation scores", some of her OCD group, in contrast to her normal controls, also made errors as regards their interim judgments of hypothesis probability (and their final choice of hypothesis). Rather than merely being over-cautious, that is, some of her OCD patients made incorrect interim estimates as to which hypothesis was most probable given the available evidence (and/or made the wrong final choice as to which hypothesis was true). Volans suggests (p316) that these findings cannot be explained in terms of "obsessionals adopting a more stringent response criterion", and she argues instead that they are better accounted for in terms of these patients being incompetent in the handling of probabilistic information. But this, then, is also to concede that her own prediction that OCD patients should be more cautious than controls on Phillips and Edwards's task is not completely consistent with her findings. Volans's results, furthermore, do not support the suggestion that an incompetence in the handling of probabilistic information is restricted to OCD patients - errors such as those made by some of her obsessional subjects were also made by some of her phobics (Volans 1976,p312).

Volans further attempted to argue that phobics should be distinguishable on Phillips and Edwards's task from both OCD patients and normal controls in terms of what she describes as their "event probability estimates" (their judgments as to how likely the first bead one is to be shown will be of one colour and how likely it is to be of the other - see sections 6.2.2.2 and 6.2.2.3 below), arguing for this position on the basis of the presence of irrational expectations in many phobic disorders. Her results from a questionnaire study and Phillips and Edwards's task provide some inconsistent support for this position (1976, p309 and p315). Despite this, however, the suggestion that phobics exhibit irrational expectancies to a greater extent than OCD patients do is surely implausible - concerns about unlikely outcomes also appear in OCD, as well as numerous other non-phobic disorders. It is also mistaken to regard as independent, as Volans evidently does, the processes of aggregating probabilistic information (concerning which she makes the predictions about OCD patients which were discussed above) on the one hand and estimating event probability (concerning which she makes her predictions about phobics) on the other. Thus, Volans predicts that phobics should be more likely than OCD patients both (1) to rule out unlikely hypotheses and (2) to have irrational expectations. Yet there is at least some risk of a contradiction here - clearly, there is a close connection between one's overestimating the likelihood of some improbable hypothesis in a given situation and one's having irrational expectations in that situation. Perhaps Volans would argue here that the putative difficulties OCD patients have in handling probabilistic information should only effect their performance on Phillips and Edwards's task

after beads have begun to be drawn, whereas the investigation of the irrational expectations of phobics was conducted before the first bead had been drawn?

Phillips and Edwards's task has since been used in investigations of deluded subjects (Huq et al 1988, Garety et al 1991) and been adapted by Garety et al (1991) to examine the reasoning of such patients with respect not only to the making of decisions but also to the maintenance or change of these decisions. The interest of these investigators in deluded patients stems from what they hypothesise to be the possible role of reasoning biases in producing delusions, their suggestion being that a tendency to leap to unjustified conclusions and/or to adhere to such conclusions in the face of contrary evidence may be features which are general to the functioning of such patients, not ones which are observed only with respect to the contents of their delusions.

The investigations by both Huq et al and Garety et al suggest that a proportion of deluded subjects do indeed exhibit the opposite profile to Volans's OCD group in making, relative to controls, both more confident interim judgments and more rapid decisions on Phillips and Edwards's task (although the differences between deluded subjects and controls on the former measure did not quite reach significance in Garety et al's study). Huq et al also found with the same task that deluded subjects exhibited overconfidence in estimating the probability of a future event (that is, the "event probability estimate" measure discussed above), although this result was not replicated by Garety et al (1991). Garety et al further reported that deluded subjects were more likely than controls to reduce their degree of confidence in an hypothesis if presented with what Garety et al termed "potentially disconfirmatory evidence" (see below) on Phillips and Edwards's task. Garety et al also reported that after having decided in favour of an hypothesis on the task, deluded subjects tended to be more inclined than controls to change their minds when presented with evidence which disconfirmed that choice (although this tendency did not quite reach significance). (These findings that deluded patients were more inclined than controls to alter their judgments might well, of course, be regarded as a surprising, given the intractable nature of delusional beliefs.)

What implications do the findings of Huq et al and Garety et al have for OCD patients? Assuming for the sake of argument that it is a disposition to develop concerns about unlikely outcomes which causes (some) deluded subjects to make both more confident interim judgments and more rapid decisions on Phillips and Edwards's task, might one not predict that at least some OCD patients will exhibit this same profile on the task? Thus, it has already been pointed out that concerns about unlikely outcomes feature in some cases of OCD. It is

also the case that it is only the presence of compulsive behaviour involving the careful performance of some action or actions which provides any grounds for Volans's prediction that OCD patients should be slower than controls in arriving at a decision on Persons and Edwards's task. Given, then, that some of the OCD patients who report concerns about unlikely outcomes do not report any compulsive behaviour, might it not be argued that the profile of these patients should correspond to that of the deluded subjects in the studies by Huq et al and Garety et al, who of course similarly report unlikely concerns unaccompanied by compulsive behaviour? The present study does unfortunately not provide a test of this prediction - no group of OCD patients who report unlikely fears but no compulsive behaviour was included.

But one might on entirely different grounds predict that some OCD patients will, in contrast to those in Volans's study and like some of the deluded subjects in the studies by Huq et al and Garety et al, make both more confident interim judgments and more rapid decisions on Phillips and Edwards's task than non OCD controls. Garety et al (1991) note that those deluded patients who tended to exhibit this profile on Phillips and Edwards's task were those who expressed a high degree of conviction in their delusions. Perhaps, then, this same feature might distinguish a subgroup of OCD patients who make confident interim judgments and rapid decisions on Phillips and Edwards's task? It has been shown elsewhere (see Chapter 7) that OCD patients differ from one another to a marked degree in terms of their reported degree of conviction in their obsessions. Perhaps, then, the tendency to take one's pathological ideas seriously could be shown to hold good across different diagnostic groups as a predictor of more confident "interim judgements" and more rapid decision making on Phillips and Edwards's task?

Following Garety et al's (1991) study with deluded patients, the present investigation of OCD patients examines the processes of belief maintenance or change, as well belief formation, with Phillips and Edwards's task. What might be predicted here? Lewis's (1936) observation that some obsessional personalities are uncertain and vacillating, others obstinate, may be relevant. There is some evidence that patients whose difficulties involve checking behaviour may tend to be uncertain and vacillating in character, whereas those whose difficulties involve cleaning tend to be obstinate (Rachman and Hodgson 1980, p54). If these dispositions of character were to be evident in the performance of these patients on Phillips and Edwards's task, one would expect both that OCD cleaners should tend to be less inclined than controls to change their minds on Phillips and Edwards's task, and that OCD checkers should be more inclined than controls to do so.

Three further points are worth noting. Firstly, and as was noted earlier, Volans (1976) used Bayesian inference as a general framework for evaluating the process of belief formation. It seems, however, that it is not the comparison with Bayesian norms, but rather that of the performance of the OCD and control groups, to which one must look in the hope of finding an explanation for why the former group exhibits symptoms not exhibited by the latter. One in effect adopts this approach if one compares, as Volans did, the extent to which the OCD and control groups deviate from Bayesian norms, using any difference in the "deviation scores" of these groups as the basis of one's account.

Secondly, and as pointed out earlier, Volans has claimed that repetitive checking can be more plausibly explained than other forms of compulsive behaviour in terms of the patient having difficulties in handling probabilistic information. Against this, it has already been argued elsewhere (see section 4.4) that the presence of unlikely concerns in both cleaning and checking difficulties means that such an explanation cannot be more plausibly applied to either one of these types of compulsion. It seems most reasonable on this basis, therefore, to make no firm prediction as to which kind of compulsion, if either, should be associated on Phillips and Edwards's task with the patient requiring more information before reaching a decision and/or tending to be conservative in his or her "interim judgments". The observation, noted above, that cleaners tend to be characteristically obstinate, checkers vacillating, is evidently also unable to support any firm prediction in this regard.

A third point, also raised in sections 5.5.1 and 5.9.1 above, concerns the performance of ex OCD patients. Reed (1985) suggests that these subjects should continue to exhibit the cognitive style described by his account, and that their performance on Phillips and Edwards's task should thus resemble that of OCD patients. To the extent that the "revised thesis" is derived from Reed's account, it too will predict that the performance of ex OCD patients on Phillips and Edwards's task should resemble that of OCD patients and differ from that of non OCD controls. This point was not addressed by Volans's study, which contained no ex OCD patients.

6.2 The present study

6.2.1 Method

6.2.1.1 Subjects

The subjects for this study were the same as those described in section 5.5.2.1.

6.2.1.2 Materials

All materials were the same as those described in Garety et al (1991). Four jars, each containing coloured beads were divided into two pairs. In the first pair, one of the jars (Jar

A) contained yellow and black beads in the ratio 85 yellow to 15 black while the other (Jar B) contained yellow and black beads in the ratio 85 black to 15 yellow (these jars were used in condition 1 - see below). The second pair of jars contained pink and green beads, one of the jars having these in the ratio 85 pink to 15 green (Jar A), the other having them in the ratio 85 green to 15 pink (Jar B) (these jars were used in condition 2 - see below). In the case of both pairs of jars, therefore, beads drawn at random from jar A would tend to be predominantly of one colour (yellow as regards the first pair of jars, pink as regards the second pair) and beads drawn at random from jar B would tend to be predominantly of another colour (black as regards the first pair of jars, green as regards the second). The subject's main task in both conditions was to judge from which jar beads were being drawn on the basis of the colour of the bead(s) he/she was shown.

In order to register estimates of probabilities, subjects were provided with a "response board" clamped to a desk to form a partial screen between the subject and experimenter. Two thin rods joined by a chain and labelled "A" and "B" were supported on cog wheels on the two sides of the response board facing the subject. Moving either of these rods a given distance up or down moved the other rod the same distance in the opposite direction (see Volans [1976] for an illustration of this set up). The position of the rods indicated the subject's estimate of the probability of the two alternatives he or she was being asked to choose between. Thus, when the rods were kept or placed at the same height by the subject his or her estimate was that the two alternatives in question were equally likely (that is, the chance of either being true was estimated to be 50:50). The more one rod was lowered, the more likely the alternative represented by that rod was indicated to be - the other alternative thereby being indicated to be correspondingly less likely, due to the second rod being automatically raised by the lowering of the first (this appears to have been the reason for including the response board device - Volans points out [p311] that "the use of connected rods forced subjects to normalise their probability estimates"). On the experimenter's side of the board a scale with 50 equal divisions was attached to the board beside each rod (the subject's side of the board was not calibrated).

6.2.1.3 Procedure

Subjects were tested individually by the ICJ during an experimental session lasting about two and a half hours. During this session they were also required to complete the control measures, a consent form (see Appendix A), and the two tasks described in Chapter 5.

Each subject took part in both conditions 1 and 2. In condition 1, subjects used the

response board to estimate how likely it was that the first bead to be drawn (see below) would be one colour or the other. In condition 2 the board was used to estimate how likely it was that one or other jar was being used (see below). Thus, marker cards indicated (in condition one) the colour of bead (yellow or black), and (in condition two) the predominant colour of beads in the jar (pink or green), represented by each rod in each of these conditions. Thus, in condition 1, rod A had a marker card reading "yellow", and rod B a marker card reading "black", and in condition 2 rod A had a marker card reading "mainly pink" and rod B a marker card reading "mainly green".

A yes/no response mode was also used in condition 1 for subjects to indicate which jar they thought to be in use. To make their responses in this mode, subjects were given two cards, one indicating that he or she had not yet reached a decision (this carried the message "more items please") and the other indicating the subject's wish to stop (this carried the message "No more items I have decided").

(These procedures for subjects to register their estimates of probabilities and record their yes/no responses are unnecessarily elaborate; as they were, however, used in previous studies they were also employed here to enable comparisons of the results of the present study with those of these earlier ones to be made.)

In each condition, the two jars to be used were placed in an opaque bag, and one of the jars picked in a manner such that this choice appeared to the subject to be random. After the choice had been made, both jars remained in the bag and the subject was informed that he or she was now to be shown beads drawn one at a time from the jar which had been selected. Three points were especially emphasised - firstly, that both jars in each condition contained beads of only two colours in the proportion 85:15, secondly that all of the beads shown to the subject in any one of the two conditions would be taken from the same jar and thirdly, that each bead would be replaced in the jar from which it had been taken after each draw. Subjects then practised the use of the rods.

The two experimental conditions were as follows:

Condition 1: As noted above, the yes/no response mode was used in this condition for subjects to indicate from which jar they thought the bead/beads they were shown had been drawn. When subjects pointed to the stop card, they were asked if they were completely sure which jar was in use; if they expressed any doubts, additional draws were offered until the subject was able to make his or her decision with certainty. On indicating that they were certain, subjects were asked which jar they thought to have been in use. Before the first draw, subjects were also required in this condition to estimate by means of the response board the

probability of a particular colour bead being drawn - to estimate, that is, how likely it was that the colour of the first bead would be yellow, and how likely it was that it would be black (the correct response being 50:50).

Condition 2: A probabilistic response mode was used in this condition for subjects to indicate from which jar they thought the bead/beads had been/were being drawn. After each draw, subjects used the response board to indicate the relative probabilities they assigned to the alternative hypotheses of the bead having come from one or other of the jars. Unlike condition 1, this part of the experiment was not terminated by the subject expressing complete certainty that one or other of the jars was being used, but rather continued for a predetermined number of draws (see below).

As in the earlier studies using Phillips and Edwards's task, the order in which subjects were to be presented with beads of one or other colour was prepared in advance for both conditions, meaning that every subject would be shown exactly the same sequence of colours. The sequence of draws in the first condition (where "Y" represents a yellow bead and "B" a black one) was as follows:

YYYBYYYYYYYYYYBYYBYYYYYYYBYYBYYYBYYY

Turning now to the second condition, the procedure adopted in the present study differed somewhat from that used by Garety et al (1991). To examine the processes of belief maintenance or change, they presented their subjects with a sequence of 20 beads, the first ten being predominately pink (suggesting Jar A to be in use), the next ten being predominately green (suggesting Jar B to be in use). This was changed for the present study because Jar A might not be judged certainly (100% likely) to be in use by all OCD patients by the time the tenth bead had been drawn. A reversal of the proportion of pink and green beads being drawn was therefore only made after the tenth bead if the subject had by that time judged Jar A to be certainly in use. If the subject had made this judgment by this point, he/she was then presented with ten beads which were, as in Garety et al's study, predominately green (as if Jar B were at this stage in use) (the order of these ten beads is shown after the brackets in the sequence below). If, however, the subject had not decided with certainty which jar was in use after the tenth draw, the initial proportion of pink and green beads continued to be drawn (the order of this continued sequence is shown in brackets below) until the subject did make this judgment with certainty. These subjects were then presented with the final ten draws of predominately green beads. All subjects who asked during the presentation of these predominately green beads if a different jar was being used were informed, consistent with the instructions they were given at the outset of the experiment, that no change had been made

(they were told that a change had been made after the experiment had been concluded.)

The sequence of draws in the second condition (where "P" represents a pink bead and "G" a green one) were:

PPPPPPGPPG [PPPGPPPGPPPGPPPPGPPGPPPPPPPPPPPPGP] GGGPGGGGPG

6.2.1.4 Measures

The measures used in the two conditions were similar to some of those used by Garety et al, being as follows:

Condition 1

(i) "draws to decision" - the number of beads the subject had to be shown before reaching a decision

(ii) "initial certainty" - the estimate of the likelihood of the first bead being yellow, and the likelihood of its being black, made prior to the first draw (this terminology follows Garety et al - this variable was that termed "event probability estimate" by Volans [1976] [see above] and "event estimation" by Huq et al [1988])

Condition 2

Belief formation stage

(i) "initial posterior estimate" - the first estimate of the likelihood of jar A having been chosen (made after the subject had been presented with the first bead)

(ii) "draws to certainty" - the number of beads the subject had to be shown before he/she was prepared to make an estimate of 100% in favour of jar A (in Volans's [1976] investigation, the results on this measure were combined with those on the "draws to decision" measure above to form a single "draws to decision" variable)

Belief maintenance/change stage

(iii) "draws to a change" - the number of beads the subject had to be shown before he/she was prepared to change his/her estimate, after having reached a certain decision - that is, the number of beads required for any change from draw 10 (or from the subject's having judged jar A 100% certain to be in use if such certainty was only obtained after draw ten)

(iv) "final decision" - the subject's last estimate, after having been shown the final bead, of the probability of jar A having been in use.

Six "additional" measures which were included in Garety et al's study were also examined here. Most concerned the performance of subjects in condition two. The first of these measures examined the effect of what Garety et al termed "potentially discomfirmatory" evidence on a subsequent estimate, this being calculated in the present investigation as the subject's estimate of the likelihood of jar A being in use in condition two after having been

shown the first six beads (all of which were pink) minus his or her estimate of the likelihood of jar A being in use after the seventh (the first green) bead. (A positive score thus shows a decrease in confidence following "potentially discomfirmatory" evidence.) This was the measure - albeit with a smaller number of pink beads preceding the first green bead - on which Garety et al found the probability estimates of their deluded subjects to be significantly more sensitive than controls to "potentially disconfirmatory" evidence (see above).

The second "additional" measure was draws to certainty in condition two that jar A had been chosen, calculated not as above but rather as in Garety et al (1991), where if an estimate of 100% had not been reached after the subject had been shown ten beads, the number of beads required for him or her to estimate at least 85% certainty for two consecutive draws was used instead.

The third "additional" measure was size of the first estimate change in condition two (calculated as the difference between, on the one hand, the subject's certain estimate in favour of jar A, and on the other, his/her estimate, at the point of his/her first change in estimate, of the likelihood of jar A being in use).

The final three "additional" measures concerned whether or not errors in "hypothesis formation" had been made on the task. The first of these measures concerned the decision made by the subject in condition one (Jar A being the correct decision), the second involved the response of the subject after the first bead had been presented in condition two (an error being regarded as any estimate in favour of jar B) and the third concerned the estimate made after the subject had been presented with the first 10 beads in condition two (in Garety et al's investigation, estimates of less than 85% in favour of jar A were regarded as incorrect; in the present study, such estimates were not so regarded if the subject eventually made a certain estimate in favour of jar A - this profile would amount to the subject's being cautious rather than in error).

6.2.2 The questions investigated by the present study

The present study was undertaken to answer several questions which have been raised or unanswered in the studies by Volans, Huq et al and Garety et al.

(i) Do OCD patients require more information than psychiatric and/or normal controls before reaching a decision and/or making a probability estimate of 100% (as indicated by the "draws to decision" and "draws to certainty" measures respectively) on Phillips and Edwards's task? And are the interim judgments of hypothesis probability made by OCD patients more cautious than those made by psychiatric and/or normal controls (as indicated by the "initial posterior estimate" measure)? Do OCD patients make (additionally or alternatively) more

errors than psychiatric and/or normal controls on the task (as indicated by any of the three "error scores" described above)?

(ii) There are a number of questions concerning whether it is only some OCD patients who may be distinguished from psychiatric and/or normal controls on Phillips and Edwards's task. Is the presence of checking as opposed to cleaning compulsions related to the performance of patients on any or all of these measures mentioned in question (i) above? Or is it rather cleaning as opposed to checking compulsions which are so related? Is there, furthermore, any tendency for cleaners to be less inclined than psychiatric and/or normal controls to alter their judgments on Phillips and Edwards's task (as indicated by the "draws to a change", "final decision", "size of the first change" and "effect of potentially disconfirmatory evidence" measures)? And is there any tendency for checkers to be in contrast more inclined than psychiatric and/or normal controls to do so? Finally, do those OCD patients who report low levels of perceived senselessness (those who have, that is, a high degree of conviction) as regards their obsessions tend to require less information than OCD patients with high levels of perceived senselessness and psychiatric and normal controls before reaching a decision and/or making a probability estimate of 100% on Phillips and Edwards's task? And are the interim judgments of hypothesis probability made by OCD patients who report low levels of perceived senselessness less cautious than those made by OCD patients with high levels of perceived senselessness and psychiatric and normal controls?

(iii) Are ex OCD patients distinguishable from OCD checkers and/or cleaners in terms of the amount of information they require before reaching a decision and/or making a probability estimate of 100% on Phillips and Edwards's task? Are their interim judgments and/or error scores similarly distinguishable from those of OCD checkers and/or cleaners? And are ex OCD patients distinguishable in any of these respects from the psychiatric (group 4) and/or normal (group 5) controls?

(iv) Do the psychiatric patients exhibit more irrational event expectancies than the OCD patients and/or normal controls on Phillips and Edwards' task? And are there any differences in this respect between the OCD checkers, OCD non checking cleaners, ex OCD patients and normal controls?

6.2.3 Results

Table 6.A shows the means and standard deviations of the groups on the "draws to decision", "draws to certainty" and "final estimate" measures.) (On the "initial certainty" measure in condition one, only nine subjects did not give a 50:50 estimate, these subjects being reasonably evenly spread across the OCD, ex OCD and non OCD patient groups; in the

case of the "initial posterior estimate" in condition two, responses were again reasonably uniform - except for group two, only two or three subjects in each group did not give a 50:50 estimate, all of these estimates being between 60:40 and 80:20 in favour of jar A. In group 2, four subjects gave similar estimates in favour of jar A, and one an estimate in favour of jar B [see below].)

One way analyses of variance showed no significant overall group effect in condition one as regards either the "draws to decision" ($F[5,47]=1.60$, $p=0.18$) or the "initial certainty" ($F[5,47]=0.58$, $p=0.72$) measures. There was similarly no overall effect for group in condition two as regards either the "initial posterior estimate" ($F[5,47]=0.54$, $p=0.75$), "draws to certainty" ($F[5,47]=1.74$, $p=0.14$) or "final estimate" ($F[5,45]=0.28$, $p=0.92$) measures.

The same contrasts which were specified in sections 5.5 and 5.9 were examined for these five variables. Unequal variances were observed across the groups as regards the "draws to decision" (Cochran's $C=0.94$, $p<0.01$) and "draws to certainty" (Cochran $C=0.84$, $p<0.01$) measures. Group 6 was therefore excluded from the analyses of the results for these packs, and the contrast results reported for them are based upon separate variance estimates (see section 5.5.3). Table 6.B shows that none of the contrasts examined were significant.

Due to Volans's finding that her psychiatric controls were on some measures distinguishable from her normal controls, these two groups were included separately in the contrasts specified on table 6.B before being combined to form a single group of "non OCD controls". None of these contrasts with the two non OCD control groups kept separate reached significance.

Given Volans's findings with her phobic group on the "initial certainty" measure, the results of the psychiatric controls (group 4) on this measure were included in further contrasts with those of the normal controls (group 5) ($t=-0.88$ with 47 df, $p=0.39$), all the OCD patients and normal controls combined (groups 1, 2, 5 and 6) ($t=1.32$ with 47 df, $p=0.19$) and all the other groups combined (groups 1, 2, 3, 4, 5 and 6) ($t=1.34$ with 47 df, $p=0.19$), none of these contrasts reaching significance.

One way analyses of variance were also conducted, along with the same contrasts as were reported in table 6.B, for results from two of the other measures taken from Garety et al's study (the effect of "potentially disconfirmatory" evidence and the alternative "draws to decision" measures). There was no significant overall group effect on either of these measures, and all of the contrasts examined also failed to reach significance.

Errors on the task were rare. Indeed, in the case of the first two "error scores", all subjects made the correct decision in favour of jar A in condition one, and only one

incorrectly judged in favour of jar B after having been shown the first bead in condition two (see below). On the third score, five OCD patients, two ex patients and one normal control judged jar A less than 85% likely to be in use after the first ten beads in condition two. As all of these subjects eventually judged jar A certain to be in use, however, none of them were in error on this measure, as defined for the present investigation (see section 6.2.1.4).

One patient (number 19) estimated jar B certain (100% likely) to be in use after having been shown the first bead in condition two. This was, therefore, both the only "one bead" certain judgment in the present investigation and, on the definition of the three "error scores" provided above, the only error on the task.

The analysis of the results on the "draws to a change" and the "size of the first estimate change" were complicated by six OCD patients, one ex patient and three non OCD controls not changing their 100% estimates in favour of jar A before the conclusion of condition two. The "draws to a change" measure was analysed by forming three categories of subject. The first contained those subjects who changed their estimates from 100% in favour of jar A by the time they had received the fifth of the last ten beads, the second contained those subjects who changed their estimates after the fifth bead but by the time they had received all of the last ten beads, and the third contained those subjects who did not change their estimate at all.

A chi square test on the number of subjects from each of these categories found across the groups revealed no overall significant difference (chi sq = 7.70 with 10 df, significance = 0.66). Certain groups were excluded from, and/or combined in, further analyses enabling the various individual groups (and combinations of groups) which were examined in Table 6.B also to be investigated on this measure. No significant differences emerged in any of these analyses.

The results of those subjects who did not alter their estimates at all were excluded for the analysis of the "size of the first estimate change" measure. Unequal variances were observed across the groups on this measure (Cochran's C = 0.62, $p<0.01$). Group six was therefore once again excluded from the analyses of the results on this measure, and the contrasts examined based upon separate variance estimates (see section 5.5.3). A significant overall group effect was observed ($F[4,34]=2.91$, $p=0.04$). The same contrasts as were reported in Table 6.B were also specified here. The cleaners (group 1) differed significantly from the non OCD controls (groups 4 + 5 combined) ($t=2.47$ with 6.1 df, $p=0.05$), and the OCD patients (groups 1 + 2 combined) differed significantly from both the non OCD controls (groups 4 + 5 combined) ($t=2.57$ with 11.4 df, $p=0.03$) and the ex OCD patients (group 3)

(t=2.31 with 12.2 df, p=0.04). Both of the OCD groups registered much larger first changes in estimate than any of the other groups.

The group means for the "draws to decision" and "draws to certainty" measures (see table 6.A) suggested that the groups of OCD patients and ex OCD patients - especially the groups of ex OCD patients - did require more information than the non OCD controls before making their judgment with certainty on Phillips and Edwards's task. As with the results of Persons and Foa's study (see tables 5.J and 5.K), therefore, the data were examined more closely to see if there were some OCD patients (or ex patients) whose results on this measure were clearly distinguishable from those of the non OCD controls. Table 6.C shows the four largest scores on the "draws to certainty" measure in each of the groups 1-3 (cleaners, checkers and ex OCD patients); the largest two scores in each of the groups 4 (psychiatric controls) and 5 (normal controls) are also shown. Each subject's number is given in brackets beside his or her score. It can be seen that two OCD patients (14 and 15) had scores which were much greater than those of the largest scores in either of the non OCD control groups, and that two ex OCD patients (25 and 22) had scores which were very much greater than the largest scores in either of the non OCD control groups (and indeed, in either of the OCD groups). One of these OCD patients (15) and one of these ex OCD patients (25) were also among those subjects who were markedly slower on some of the packs of cards in Persons and Foa's task. The other two subjects had unexceptional scores on Persons and Foa's task; they were both women, the OCD patient (14) was a checker and the ex patient (22) had been such when ill. The Mill Hill and BDI scores of the former were unremarkable; both scores were low in the case of the latter.

The lowest scores of the OCD patients on the "draws to certainty" measure were very like those of the non OCD controls, with the exception of subject 19 (see above).

The largest and smallest scores in each of the groups on the "draws to decision" measure were reasonably similar with just one exception, subject 25, who on this measure once again had by far the largest score in the whole sample.

Turning now to the question of whether levels of perceived senselessness were related to the performance of subjects on Phillips and Edwards's task, it will be recalled that the sixteen OCD patients who formed the second group in the second cluster analysis reported above (see section 2.2.3.2) were distinguished from the thirty six patients included in the first cluster in reporting low levels of perceived senselessness (and low levels of resistance) as regards their obsessions and compulsions. The results of the second group from this cluster analysis were compared with those of the first group and the non OCD controls on the "draws

to decision", "initial posterior estimate" and "draws to certainty" measures. Unequal variances were observed across the groups as regards all three of these measures ("draws to certainty" Cochrans C = 0.93[p<0.01], "initial posterior estimate" Cochrans C = 0.4[p=0.05], "draws to certainty" Cochrans C = 0.56[p<0.01]), and so the contrasts examined were based upon separate variance estimates. There was no significant difference on any of these measures between the second group from the second cluster analysis and either the first group from that analysis, or the psychiatric and normal controls.

As in sections 5.5.3 and 5.9.3, the results of the 8 non cleaning checkers from group 2 were examined. Their results were the same on all measures from those of group 2 as a whole. Analyses were also conducted with neuroticism partialled out on those variables ("draws to a decision" and "draws to certainty") where Volans had similarly partialled out neuroticism to produce a significant group effect. No significant group effect was produced by this manoeuvre in the present investigation.

Table 6.A: The means and standard deviations of the groups on the "draws to decision" (condition 1), "draws to certainty" (condition 2) and "final estimate" (condition 2) measures on Phillips and Edwards's task

	Draws to decision	Draws to decision	Draws to certainty	Draws to certainty	Final estimate	Final estimate
	M	SD	M	SD	M	SD
All OCD pts (N=23)	3.74	1.91	7.13	5.60	60.91	34.07
Cleaners (N=10)	4.20	1.87	7.30	4.62	56.50	39.37
Checkers (N=10)	3.70	2.11	7.90	7.16	67.50	32.60
Ex OCD pts (N=10)	10.40	15.20	16.50	21.58	61.67 *	30.52
Non OCD ctls (n=20)	3.65	1.60	5.35	2.83	54.25	29.07

* = One case missing

The "final estimate" results refer to the subject's judgment of the likelihood of jar A being in use.

Note: "pts" = patients, "ctls" = controls

Table 6.B: Contrast results for the "draws to decision" and "initial certainty" (condition 1), and "initial posterior estimate", "draws to certainty" and "final estimate" (condition 2) measures on Phillips and Edwards's task - OCD patients, ex OCD patients and controls

	Initial certainty	Draws to decision	Initial posterior estimate	Draws to certainty	Final estimate
All OCD pts vs non OCD ctls	t=1.00,p=0.32	t=0.52 with 33.2 df,p=0.61	t=-0.91,p=0.37	t=1.52 with 21.9 df,p=0.14	t=0.33,p=0.75
Checkers vs cleaners	t=0.14,p=0.89	t=0.56 with 17.8 df,p=0.58	t=0.55,p=0.58	t=-0.22 with 15.4 df,p=0.83	t=-0.76, p=0.45
Checkers vs non OCD ctls	t=0.10,p=0.92	t=0.07 with 14.5 df,p=0.95	t=-1.45,p=0.15	t=1.09 with 10.4 df,p=0.30	t=1.06,p=0.30
Cleaners vs non OCD ctls	t=0.26,p=0.79	t=0.79 with 15.9 df,p=0.44	t=-0.81,p=0.42	t=1.23 with 12.4 df,p=0.24	t=0.18,p=0.86
Ex OCD pts vs all OCD pts	t=0.20,p=0.84	t=-1.34 with 9.2 df,p=0.21	t=-0.74,p=0.47	t=-1.28 with 9.7 df,p=0.23	t=-0.26, p=0.80
Ex OCD pts vs non OCD ctls	t=-0.65,p=0.52	t=-1.40 with 9.1 df,p=0.19	t=0.02,p=0.99	t=-1.63 with 9.2 df,p=0.14	t=-0.57, p=0.57
Ex OCD pts vs checkers	t=-0.47,p=0.64	t=-1.38 with 9.3 df,p=0.20	t=-1.24,p=0.22	t=-1.20 with 11.0 df,p=0.26	t=0.39,p=0.70
Ex OCD pts vs cleaners	t=-0.33,p0.74	t=-1.28 with 9.3 df,p=0.23	t=-0.69,p=0.50	t=-1.32 with 9.8 df,p=0.22	t=-0.35, p=0.73

All t values quoted have 47 df unless otherwise stated; all t values for "final estimate" have 45 df

Note: "pts" = patients, "ctls" = controls

Table 6.C: Highest scores in each of the groups 1-5 on the "draws to certainty" (condition 2) measure on Phillips and Edwards's task

	1st	2nd	3rd	4th
Cleaners	14 (6)	13(9)	12 (1)	10 (8)
Checkers	22 (14)	19 (15)	9 (11)	9 (18)
Ex OCD pts	69 (25)	41 (22)	12 (30)	11 (24)
Psychiatric ctls	9 (31)	9 (33)		
Norm ctls	11 (46)	11 (47)		

Note: "pts" = patients, "ctls" = controls, "norm" = normal

6.3 Discussion

Answers can now be provided to the four questions raised earlier. Taking each of these in turn, the present study found that:

(i) The OCD patients (groups 1, 2 and 6 combined) were indistinguishable from psychiatric controls (group 4) and normal controls (group 5) in terms of the amount of information they required either before reaching a decision or before making a probability estimate of 100% on Phillips and Edwards's task. The interim judgments of hypothesis probability of the OCD patients on the one hand, and the psychiatric and normal controls on the other, were also indistinguishable, as were the error scores of these groups.

(ii) The cleaners (group 1) and the checkers (group 2) were indistinguishable both from one another and from the psychiatric (group 4) and normal (group 5) controls in terms of the amount of information they required either before reaching a decision or before making a probability estimate of 100%, in terms of their interim judgments of hypothesis probability and in terms their error scores. The cleaners (group 1) and the checkers (group 2) were similarly indistinguishable both from one another and the psychiatric (group 4) and normal (group 5) controls in terms of how prepared they were to alter their judgments on Phillips and Edwards's task, as indicated by the "draws to a change", "final decision" and "effect of potentially disconfirmatory evidence" measures. The "size of the first estimate change" measure indicated that, among those subjects who did change their estimate, the OCD patients (groups 1 and 2 combined) were inclined to make larger initial changes than either the ex OCD patients (group 3) or the non OCD controls (groups 4 and 5). The two groups of OCD patients were indistinguishable from one another in this respect. The reported level of perceived senselessness among OCD patients as to their obsessions and compulsions was not related to the amount of information they required to reach a decision or make a probability estimate of 100% on Phillips and Edwards' task. These patients' reported level of perceived senselessness was also unrelated to their interim judgments of hypothesis probability.

(iii) The ex OCD patients (group 3) were indistinguishable from the cleaners (group 1) and the checkers (group 2) in terms of the amount of information they required before reaching a decision or before making a probability estimate of 100% on Phillips and Edwards' task, in terms of their interim judgments of hypothesis probability and in terms of their error scores. The ex OCD patients were also indistinguishable in all these respects from the psychiatric (group 4) and normal controls (group 5).

(iv) All of the groups examined were indistinguishable in terms of their event expectancies.

Taking all of these results together, then, they provide almost no evidence in favour of either Reed's account or the "revised thesis". Some very limited support for these positions was provided by the two OCD patients (both checkers) and two ex patients (both former checkers) who were clearly distinguishable from all the non OCD controls in terms of the amount of information they required to make a certain estimate on Phillips and Edwards's task. As with the results from Persons and Foa's task, however, even this support must be considered in the context of the theoretical problems raised elsewhere (section 4.4) for Reed's account as applied to at least many cases of checking symptoms, as well as the more limited objections which were also brought against the "revised thesis" as applied to such symptoms.

Almost no support has been provided for the numerous hypotheses examined in this investigation which were not derived either from Reed's account or the "revised thesis". On the "size of the first estimate change" measure, all OCD patients were more inclined than the non OCD controls (and the ex OCD patients) to alter their judgments. It is perhaps not clear what this isolated finding means (there were no group differences on any of the other measures concerning how prepared subjects were to change their estimates), and the finding in any case partially disconfirms one prediction - that cleaners should be less inclined than non OCD controls to alter their judgments on the task - as well as partially confirming another - that checkers should be more so inclined than controls.

In contrast to the performance of deluded subjects observed by Huq et al and Garety et al, only one subject in the present investigation made a 100% estimate after having been shown only one bead, and even this subject made her 100% estimate in favour of the jar containing beads predominantly of the opposite colour to that which she had been shown, in contrast to Huq et al's and Garety et al's subjects.

The discrepancies between the present findings and those reported by Volans (1976) may be difficult to explain, but it is clear that the present study is the more powerful test of the hypotheses examined by Volans, having included over three times as many OCD patients as Volans's investigation (nearly five times as many if one counts the ex OCD patients in the present investigation). Even if Volans's results turned out to be replicable, furthermore, an alternative explanation of them might be presented. Thus, might it not be that OCD patients could produce such results because of a concern about being criticised for making errors on this task, an interpretation of these results which would probably be consistent with several of the accounts considered earlier (see chapter 3)? (It will be recalled that the same explanation was also considered as regards both Persons and Foa's task and the "Essentials" test [see sections 5.6 and 5.10].) It is indeed surely clear that such a concern might well lead

to OCD patients being more cautious on Phillips and Edwards's task, and requiring more information before reaching a decision and making a probability estimate of 100% on the task, although perhaps not to their making more errors, as Volans also reported her OCD and phobic groups to do. Even if findings such as Volans's with Phillips and Edwards's task were replicable, therefore, this alternative account would need to be checked as a possible explanation of some of these results (although the suggestion that OCD patients have difficulties in dealing with probabilistic information might still be consistent with this alternative account - these difficulties would lead these patients, it might be suggested, to overestimate the likelihood of their being blamed for making mistakes). The present findings, of course, suggest that no such effect as that described in this alternative account is taking place.

6.4 Summary

The present study provides little evidence in favour of Reed's account or the "revised thesis". A number of other hypotheses derived from sources independent of these theories were also not supported. An alternative explanation of findings such as Volans's was suggested which, in conjunction with some remarks regarding experimental tasks in general which were offered elsewhere (section 4.8), suggest that the task could by itself not provide very strong evidence in favour of Reed's account or the "revised thesis", whatever the findings reported.

6.5 Part C conclusions

The main conclusions of Part C may be briefly stated. Very little support either for Reed's account or the "revised thesis" has been provided by the foregoing investigations with Persons and Foa's task, the "Essentials" test and the "probabilistic inference" task devised by Phillips and Edwards. This conclusion may be linked in a most straightforward way with the conclusion of the theoretical discussion of Reed's account offered in the previous chapter. There it was suggested that no reason had been found for believing that the thinking style described in Reed's account plays any role in producing the distress observed in obsessional disorders. The present chapter suggests that this thinking style may well not even be observed as regards the functioning of at least the great majority of OCD patients on the tasks examined. One is entitled, therefore, to regard Reed's account with considerable scepticism, even in the partial role it was suggested might be open for it at the conclusion of the previous chapter.

Part D: Concluding Remarks

Chapter seven: Conclusions and suggestions for future work

7.1 Introduction

To sum up all that has been said so far: the standard diagnostic criteria for OCD have been challenged, and an alternative analysis proposed which suggests that no single feature or collection of features is both common and peculiar to all instances of the disorder. Consistent with this position, it was also argued that there is no single way in which OCD may be distinguished either from phobias or from delusional states, and various different ways in which, it was suggested, OCD may be so distinguished were presented. An empirical investigation of the characteristics of clinical obsessions and compulsions provided still more evidence against the standard diagnostic criteria, confirming that not all such obsessions and compulsions are experienced as senseless or are resisted. A cluster analysis further revealed a subgroup of OCD patients marked out by low levels of both perceived senselessness and resistance with regard to their obsessions and compulsions. It has been further suggested that the difficulties with which all of the theoretical accounts of OCD considered in the foregoing chapters meet are more striking than their successes. While some of these accounts make observations concerning OCD patients which may prove to be important, it has been suggested that these observations could not support anything which would even approach a full explanation of OCD.

de Silva (1988, p195 and p202-3) suggests that "the limited progress that has been made in our understanding [of OCD]" may be partly due to "the tendency...to assume that [OCD represents] a unitary phenomena with each theoretical model being offered to account for a diversity of symptoms and presentations". de Silva argues that this "unitary" view may be mistaken, and suggests one or two phenomenological distinctions which may, he argues, be important from the point of view of the explanation of OCD. Yet the importance of any phenomological distinction to the explanation and/or treatment of OCD remains an open question - it may be that there are no important subdivisions of OCD from the point of view of explanation or treatment, or (perhaps more plausibly) that such subdivisions do not correspond to any differences in the kind of symptoms reported. Insel (1982) suggests one such subdivision based on the age of onset of the disorder; another subdivision, related more to the character disposition of the patient than to the nature of his or her symptoms, will be tentatively suggested below.

A satisfactory explanation of OCD does not, then, emerge from the various theoretical approaches to the disorder examined above. So can any of the work which has been reviewed

be said to be making progress towards an explanation of OCD? It is necessary to be clear as to exactly what this question means. Lakatos (1970) distinguishes between "progressive" and "degenerating" scientific research. On Lakatos's account, research is "theoretically progressive" if it leads to new predictions, and "empirically progressive" if these predictions are successful. "Degenerating" scientific research, in contrast, is described by Lakatos as being that which is yielding neither new predictions nor empirical successes. Such research will also have had its former empirical successes (if any) accounted for by some rival approach, and this rival approach will, on Lakotos's account, have had empirical successes of its own which the "degenerating" approach will only be able to "explain" by resorting to "ad hoc manoeuvres" (Gholson and Barker 1985). Putnam (1974) provides an important qualification of Lakatos's view when he suggests that the success of a scientific theory may consist in the elegant or "striking" explanation of some already known fact instead of (or as well as) the correct prediction of some novel finding. Similarly, what is important about a theory is surely not that it leads to new predictions and findings, but rather that these predictions and findings are such that rival theories cannot plausibly explain them. This point may be implicit in Lakotos's further suggestion that the predictions made by "progressive" research should be "stunning" (Lakotos 1970).

Putnam believes himself to be giving an account of Kuhn's (1962) notion of a "paradigm" in the remarks quoted above while Lakatos, on the basis of his (1970) criticisms of Kuhn's work, is stating the conditions for progress in what he describes as a "research programme" (that is, a succession of theories linked by a common core of shared ideas, this core not itself being open to immediate empirical refutation - see Lauden [1977] for some criticisms of this position). It is at best unclear whether any of the work on OCD described above is substantial enough to count as either a "paradigm" or a "research programme". At the risk of misusing the ideas of Lakatos and Putnam, however, let us suspend judgment here as to whether any of this work could be so regarded, and ask only if any of the accounts of OCD presented may be said to be "progressive", insofar as they have produced a good explanation of existing observations and/or have predicted some novel finding of which other approaches would have difficulty in making sense.

It has been argued by Eysenck (1977) that the behavioural approach to OCD is indeed "progressive", in the terms of Lakotos's work, and Eysenck specifically cites in defence of this claim the results of exposure with response prevention in the treatment of the disorder. He argues that these results provide a stunning, or at least psychiatrically important, confirmation of a prediction made by the behavioural approach.

But against the suggestion that this confirmation makes the behavioural approach "progressive", and leaving to one side the question of the efficacy of exposure with response prevention, there are various alternative explanations available as to why this intervention might help some OCD patients. Gray (1979, 1982), for example, has put forward an "innate fear" account of OCD (see section 3.3.5). While this account meets with difficulties of its own, it is able to explain both the treatment results Eysenck cites in favour of the behavioural approach, as well as a number of further findings with which that approach is arguably only able to deal by the introduction of "ad hoc manoeuvres", for example Eysenck's explanation in terms of his notion of "incubation" of fear eliciting stimuli not undergoing extinction (see section 3.2.2). This suggests, then, that contrary to Eysenck's position, the behavioural account of OCD is at present "degenerating", in Lakatos's sense of this term.

There are, furthermore, still other accounts of the mechanisms by which exposure with response prevention may help some OCD patients, accounts which are derived from still other proposed explanations of OCD, for example, the poor "spontaneous structuring" of experience (see section 4.7), low "perceived self efficacy" (see below) and displacement/symbolisation (see below).

Can an example be provided, then, from among the theoretical approaches to OCD discussed in the foregoing chapters, of an account which, while being far from complete, is "progressive", in the sense outlined above? It was noted in connection with both Janet's discussion of OCD (section 3.4), and the psychodynamic case discussion presented by Malan (section 3.6), that a tendency to unassertiveness is observed in some OCD patients. Authors such as Janet (1903), Malan (1979) and Emmelkamp and van der Heyden (1980) have all suggested that a failure to act assertively and/or to express aggressive feelings may help precipitate the symptoms of some OCD patients, and that these symptoms may, therefore, be alleviated by an intervention which successfully encourages these patients to behave more assertively and/or to express their aggressive feelings to a greater extent (Claridge [1985] also offers a few suggestions along these lines - see section 3.3.7.2). An attempt will now be made to show that these suggestions are "progressive", both in "theoretical" and "empirical" terms.

An intervention which encourages patients to behave more assertively and/or to express their aggressive feelings to a greater extent is "assertion training". What exactly is the evidence, then, in favour of this intervention's helping to reduce the symptoms of those OCD patients who are unassertive? Such evidence would suggest Emmelkamp and van der Heyden's position, and those aspects of Janet's and Malan's accounts which coincide with it, to be "empirically progressive". And, if there is any such evidence, can it further be shown that, in

contrast to the success claimed for exposure with response prevention, this evidence is such that it is at least difficult to reconcile with accounts of OCD other than those used here to support this prediction? Can, that is, one argue that a favourable response of patients to this intervention is indeed likely to result from its increasing their level of assertiveness as such, rather than having to do with mechanisms which could be more easily accommodated within accounts of OCD which make no reference to unassertive behaviour or unexpressed aggressive feelings as precipitants of some OCD symptoms? This demonstration is closely linked to the question of whether Emmelkamp and van der Heyden's account, and again, those aspects of Janet's and Malan's with which that account overlaps, may be said to be in the present respect "theoretically progressive".

So far, only Emmelkamp and van der Heyden have reported a controlled investigation of the effectiveness of assertion training with OCD patients. In what follows, this investigation will be discussed at some length, in an attempt to provide as complete an answer as is possible to the above questions. Other implications of Emmelkamp and van der Heyden's findings, and further work which these findings suggest may be worth conducting, will also be explored.

7.2 Emmelkamp and van der Heyden on assertion training

Emmelkamp and van der Heyden (1980) reported a comparison of assertion training and thought stopping as treatments for "harming obsessions", that is obsessions the contents of which concern "harming oneself or harming others" (Emmelkamp and van der Heyden 1980, p29). These authors suggest that "research into the treatment of obsessions should take into account the differences in the content of various obsessions" (1980, p29) and on these grounds make out their case for using assertion training with harming obsessions. Thus, Emmelkamp and van der Heyden note that patients with such obsessions are, like the patients discussed by Janet and the case presented by Malan, inclined to be unassertive and that, at least in some cases, the most powerful precipitants of such obsessions are situations which require these patients to act assertively. One such situation which Emmelkamp and van der Heyden give as an example is that of being criticised unfairly by someone, which in the case of one patient they treated gave rise to obsessional thoughts about harming the person by whom the patient had been criticised. Emmelkamp and van der Heyden also point out that most patients in their study "reported an increase of their obsessions during unresolved interpersonal conflict" (1980, p33). The obsessions of these patients were therefore formulated by these authors as resulting from "unexpressed aggressive feelings and the associated guilt" (1980, p29, original emphasis) concerning such feelings. These authors also rather speculatively assert that this hypothesised guilt at aggressive feelings is "a result of [these

patients'] upbringing" (1980, p29). Emmelkamp and van der Heyden predict that assertion training should help these patients by leading to "a more adequate handling (i.e. expression) of aggression", producing a reduction in "guilt feelings and hence [a reduction in] obsessions" (p29).

To examine the effects of assertion training and thought stopping, a cross-over design was employed, enabling the comparison of these two treatments to be made both within and between subjects. Six patients, all women, were included in the study, half receiving thought stopping prior to assertion training, half receiving these two treatments in the reverse order.

Emmelkamp and van der Heyden's statements regarding the outcome of their study are in some measure equivocal. Thus, they report that no significant differences between the two treatments were found at post treatment and that assertiveness training is merely "at least as effective as thought stopping" (p28). They also report, however, that "the results of the thought stopping procedure were not encouraging" and that "assertiveness training led to a...reduction of the frequency of obsessions for most patients" (p33), enabling them also to state that "most patients seemed to have benefitted more from assertiveness training than from thought stopping" (1980, p28). The justification for these claims as to the superiority of assertion training is that it led to a reduction in the frequency of obsessions in at least four (and probably five) of their cases, whereas improvement occurred after the administration of thought stopping in only two cases. One may evidently conclude that there is some, although by no means conclusive, support here for Emmelkamp and van der Heyden's more optimistic remarks concerning the results of assertion training with their OCD patients.

Assertion training also led to a significant increase in assertive behaviour, Emmelkamp and van der Heyden report. This was despite assertiveness scores at the pretreatment stage being artificially high because of two patients who were "scoring within normal ranges [of assertiveness]" at this stage due to "a lack of insight into their interpersonal problems" (Emmelkamp and van der Heyden 1980, p33).

7.3 Alternative explanations of the effects of assertion training

7.3.1 Introduction

To the extent that these findings suggest assertion training to be of some therapeutic value for some cases of OCD, do they support only hypotheses such as Janet's, Malan's or Emmelkamp and van der Heyden's as to the genesis of obsessions of the type in question? Are there not alternative explanations of the mechanisms by which an assertion training intervention might help some cases of OCD, explanations which are consistent with alternative accounts of how such obsessions may be produced? Several such alternative explanations of

Emmelkamp and van der Heyden's findings will now be considered.

7.3.2 Boyd and Levis's critique

Boyd and Levis (1983) draw attention to the fact that Emmelkamp and van der Heyden have been dealing with obsessions the contents of which involve the theme of hurting other people. Boyd and Levis correctly point out that, for such cases of OCD, contact with the people whom the patient fears he or she may harm (as would be required by assertion training exercises) could be operating via the same mechanisms as are involved in exposure with response prevention to produce the therapeutic effects reported. And as argued above, the therapeutic success of exposure with response prevention is probably consistent with many of the theoretical approaches to OCD considered in the foregoing chapters - on Boyd and Levis's interpretation, in other words, many of these approaches would probably be consistent with Emmelkamp and van der Heyden's findings.

A powerful test of this interpretation would be to treat with assertion training patients whose symptom contents do not involve contact with other people at all. It should be possible to carry out such a test - clinical experience suggests Emmelkamp and van der Heyden's claim that research should take into account the differences in the content of various obsessions probably places too much emphasis on the importance of symptom content. Indeed, Emmelkamp (1982) himself has since made this point. Thus, patients whose problems concern, for example, checking plug sockets sometimes report difficulties in asserting themselves as an important precipitant of their obsessions. Using assertion training to treat patients such as these would not encounter Boyd and Levis' objection.

It is in any case important to note that Boyd and Levis fail to acknowledge several of the most important strengths of Emmelkamp and van der Heyden's argument which do not concern the mechanisms by which assertion training may be helping patients with harming obsessions. Thus, the observation that their patients were unassertive prior to therapy, and that at least some of these patients had their obsessions provoked by situations which required them to be in some measure assertive, both provide support for Emmelkamp and van der Heyden's position. Any convincing reinterpretation of Emmelkamp and van der Heyden's study should be able to make sense of these observations, and Boyd and Levis make no attempt to do this.

Boyd and Levis also point out that several of the patients in Emmelkamp and van der Heyden's study also received exposure with response prevention as a treatment for their obsessions, and offer this as a further objection to Emmelkamp and van der Heyden's account. But this criticism appears to be based on a misunderstanding of Emmelkamp and van der

Heyden's paper. Exposure with response prevention was only given after the period during which assertion training was tried.

It is also worth noting that Boyd and Levis's explanation would probably differ from Emmelkamp and van der Heyden's in its implications for the experience of the subject taking part in assertion training exercises. Boyd and Levis's hypothesis would suggest that these subjects should experience their obsessions early on in such exercises, these obsessions reducing in time both within and between sessions, as a result of exposure to provoking cues during the assertion exercises. Emmelkamp and van der Heyden's account probably has contrasting predictions. If obsessions are as they say the result of such situations as the patient's failing to deal appropriately with criticism, and assertion exercises involve the patient in dealing more successfully with such situations, then the obsessions would be most likely, on this hypothesis, not to occur at all during these exercises, or at most to occur only in a milder form. Unfortunately, Emmelkamp's and van der Heyden's discussion does not enable one to determine which if either of these alternatives was the usual experience for their patients.

7.3.3 Another "exposure based" account?

A different approach to that of Boyd and Levis leads to the apparently similar claim that exposure may be the mechanism by which assertion training operates, and may thus similarly appear to suggest that various theoretical approaches will be consistent with Emmelkamp and van der Heyden's findings. This approach would argue that what the patient is exposed to in such training are the consequences of asserting himself. The patient, according to this approach, discovers in assertion training that it is, for example, possible to refuse the demands of others, or make reasonable demands of them, without spoiling all subsequent contact with them.

But to describe the foregoing as an "exposure" based account of how assertion training may work is at best misleading. The account clearly shares much in common with Emmelkamp and van der Heyden's original interpretation, and in particular, like that interpretation, makes the crucial anti-exposure suggestion that patients are not being helped in assertion training by being brought into contact with the contents of their symptoms (these contents involve the theme of harming others by attacking or insulting them, not by making or refusing demands in an appropriate manner). The account is thus best regarded, despite appearances, as a restatement or expansion of Emmelkamp and van der Heyden's own position as regards the mechanisms by which assertion training works.

7.3.4 Assertive behaviour as reciprocal inhibition

Wolpe (1958) proposed another explanation for the therapeutic effects of assertion training (as applied to anxiety disorders in general, rather than specifically to OCD). This explanation is in terms of "reciprocal inhibition" - assertive behaviour serves to reduce anxiety by being incompatible with it, in much the same way that, for example, relaxation exercises, sexual arousal or feeding would also be held by Wolpe to reduce anxiety. This proposal, if correct, would suggest that the success of assertion training in treating harming obsessions would perhaps be consistent with a variety of different approaches to OCD, and would certainly be consistent with the behavioural account (see section 3.2), to which Wolpe himself subscribes.

Like Boyd and Levis's explanation, however, Wolpe's account fails to explain the wider picture of unassertive behaviour leading to the appearance of symptoms prior to treatment and would thus not provide a convincing interpretation of the success of assertion training with patients such as Emmelkamp and van der Heyden's.

7.3.5 Assertive behaviour and perceived self efficacy

Yet another alternative explanation might make better sense of this wider picture. Bandura (1978) argues that a wide range of anxiety disorders including OCD and phobias result from the patient's belief that he is unable to cope with whatever it is that he fears - the patient has "low perceived self-efficacy" to use Bandura's (rather clumsy) phrase, as regards the stimuli and situations he fears. Bandura also argues that all behavioural treatments work by increasing "perceptions of self-efficacy" - Bandura would, for example, suggest both that a spider phobic's fundamental problem is his belief that he is unable to deal with spiders and that exposure with response prevention works with such a patient by enabling him to discover that he can deal with them and/or by his acquiring means by which to do so. The "increased perceptions of self-efficacy" this produces may generalise, Bandura suggests, leading to improvements elsewhere in the patient's functioning, for example regarding fears with entirely unrelated contents suffered by the same patient (Bandura 1978).

Applying this approach to assertion training (Bandura himself has not done this), it would be argued that this intervention, if successful, would cause an increase in the "perceptions of self-efficacy" of patients as regards their interaction with other people. Any improvement in the obsessions of patients such as Emmelkamp and van der Heyden's as a result of assertion training would furthermore be seen, on this approach, to be due to the generalisation of these increased "perceptions of self-efficacy" to the patients' symptom areas, in a manner similar to that in which Bandura argues that such perceptions sometimes

generalise from one phobia to another.

A full evaluation of Bandura's self efficacy hypothesis is beyond the scope of the present discussion, but a few points regarding this interpretation of Emmelkamp and van der Heyden's study are worth noting. This interpretation, unlike Boyd and Levis's and that taken from Wolpe's work, does not take issue with Emmelkamp and van der Heyden's claim that assertion training has reduced their patients harming obsessions via its effects on these patients' levels of assertiveness. Nonetheless, the interpretation does question the mechanisms by which Emmelkamp and van der Heyden suggest this effect is brought about, and may thus also be used to deny the role which Emmelkamp and van der Heyden assign to unexpressed aggressive feelings (and associated guilt) in producing "harming obsessions" in the first place. (It seems that this "perceived self efficacy" interpretation of Emmelkamp and van der Heyden's findings would probably also not provide strong support for any of the other accounts of OCD reviewed in the foregoing chapters.)

There are a number of difficulties facing this application of the "perceived self efficacy" account. It is probably too ambitious to attempt an account of Emmelkamp and van der Heyden's study in exactly the same terms as those in which Bandura already tries to explain such a wide range of other phenomena. Emmelkamp and his colleagues, for example, have also used assertion training with agoraphobics (Emmelkamp et al 1983) and found that improvements in the assertiveness of these patients tended to occur independently of any changes in their symptoms. Why, then, does the hypothesised improvement in "perceptions of self-efficacy" generalise from the patients increased assertiveness to symptom contents in the case of "harming obsessions", but fail to do so in the case of agoraphobia? The unitary explanation of these different disorders and their treatment in terms of "perceptions of self-efficacy" is probably silent as to this question - a question which does not arise for Emmelkamp and van der Heyden's more specific account from which no prediction as to the effects of using assertion training with agoraphobics can be derived. It is, furthermore, difficult to see how some harming obsessions could be the result of perceptions of low self-efficacy, and why, therefore, they should be eliminated by any hypothesised increase in such perceptions. What, for example, would such an account make of the impulse to harm others? (Note in particular that such a symptom is not a doubt that one may have harmed others, which could perhaps be represented as "low perceived self-efficacy" as regards one's self control.) Perhaps it would be suggested that distress results not from the impulse per se but from the patient's "low perceived self-efficacy" regarding his ability to hold the impulse under control? But this move renders the account rather like that presented by Salkovskis (1985) as

regards OCD in general, objections to which have already been offered elsewhere (Jakes 1989[i], 1989[ii]). Perhaps doubts that one may have harmed others might be argued instead to produce the impulse to act in this manner, as Gray (1982) has suggested? But again, there is at least nothing in Bandura's "perceived self-efficacy" account which would explain why this should be so, while the account within which Gray himself makes this suggestion has been criticised earlier (see section 3.3.6).

7.3.6 Conclusion

Four alternative accounts as to the mechanisms by which assertion training may have helped some of Emmelkamp and van der Heyden's patients have now been considered. Three of these accounts would have been consistent with explanations other than that offered by Emmelkamp and van der Heyden themselves of how "harming obsessions" arise. It has been suggested, however, that all three of these accounts are probably less plausible than the position defended by Emmelkamp and van der Heyden (although this position faces difficulties of its own - see below), while the remaining alternative turned out on examination to be a restatement of that position. The present discussion provides, therefore, some support for the suggestion that the successful use of assertion training with some cases of OCD could not be plausibly explained by a variety of different theoretical approaches to the disorder. Assertion training can in this respect, therefore, be contrasted with exposure with response prevention.

7.4 Shortcomings of Emmelkamp and van der Heyden's study.

Emmelkamp and van der Heyden's study has a number of important shortcomings which should be borne in mind when considering its findings. As noted, only six patients were included, and each of these received both assertion training and thought stopping. This design, as Emmelkamp and van der Heyden themselves point out, is only able to answer questions as to short-term therapeutic effects - a between group study would be required to assess the long term effectiveness of either intervention.

The use of larger numbers of patients should be made possible by something which was pointed out earlier - Emmelkamp and van der Heyden have probably taken symptom content as too important an indicator as to underlying pathology and it may be possible, therefore, to help OCD patients other than those with harming obsessions by means of this intervention. Of equal importance, and as noted earlier (for example, section 3.4.5.2), some OCD patients are overassertive and rigid in their dealings with others, precisely the opposite profile to that observed among Emmelkamp and van der Heyden's patients. This wider profile of the patient's level of assertiveness may be the most important indicator as to the likely value of assertion training for him/her, not the nature of his/her symptom contents (this point

calling to mind the suggestion in section 7.1 above that the most important subdivisions of OCD may not have to do with phenomenology or symptom types at all).

Emmelkamp and van der Heyden's study would have been improved by assertion training's being compared with a more established treatment than thought stopping. Their conclusion that "most patients seem to have benefitted more from assertion training than from thought stopping" in itself tells us little as to the value of assertion training, given the uncertain status of thought stopping - at most this entitles one to conclude that assertion training is more effective than an alternative which might have some face validity for the patient, but is otherwise an unknown quantity. (Emmelkamp and van der Heyden's finding that improvement occurred in at least four of their six cases after assertion training had been administered nonetheless provides, as suggested earlier, some grounds for encouragement.)

It is also worth repeating that while unassertiveness and unexpressed aggressive feelings may be important to our understanding of some cases of OCD, a sufficient explanation of the disorder can obviously not be provided in these terms (also see section 3.6.4.3). Unassertiveness or unexpressed aggressive feelings can similarly not be used to explain why one person develops OCD while another develops, for example, phobic symptoms, given that such difficulties are common in both disorders.

7.5 Assertiveness training and exposure therapy

It has been proposed above that some OCD patients who do not experience harming obsessions, suffering instead from, for example, cleaning and checking problems, might be successfully treated with assertion training. The nature of the symptoms of many such patients would mean that they might also be treated with exposure with response prevention. It has also been noted that some of Emmelkamp and van der Heyden's patients were reported to have been successfully treated with exposure with response prevention after assertion training and thought stopping had been compared. These observations raise the question of how one might make sense of <u>both</u> of these treatments helping some patients - to what extent, that is, might one suppose that these interventions operate by different mechanisms, and how might the operation of these mechanisms, given that they are different, be supposed to interact (if they do so at all) in such cases?

Clinical experience with OCD patients raises some points as regards these questions (it is stressed that this experience only <u>raises</u> these points, it does not provide strong support for them). Consider an example. The major OCD symptom of a patient treated by the author was compulsive checking, especially of certain aspects of her appearance, for asymmetry. A programme of exposure with response prevention was introduced for her, and within a few

weeks her checking was substantially reduced. This reduction seemed to make the patient more aware of the precipitants of her symptoms and these seemed in her case often to be situations in which she felt required but unable to make requests of, or refuse requests from, family members, friends and strangers.

This process of these situations provoking OCD symptoms was described by this patient as her distress concerning such situations "becoming lost" in her checking; engaging in exposure with response prevention, she reported, caused such distress "to remain where it belonged". On her account, that is, she became clearer, as a result of exposure with response prevention, of the genuine source of her distress (her inability to assert herself) instead of this distress finding its expression elsewhere in her functioning (the checking behaviour which her therapy required her not to carry out). This patient similarly reported herself to have been less aware, prior to taking part in exposure with response prevention, that she had hitherto not been able to make demands, and to deal with the demands, of others in a manner satisfactory to herself.

Given that exposure with response prevention had left this patient no less uncomfortable about making demands and dealing with the demands of others, assertion training was a very natural additional intervention for her. "Assertion training" in this case amounted to no more than such straightforward tactics as discussing appropriate ways in which various requests might be politely made or refused, in conjunction with "homework" assignments in which these suggested means of requesting and refusing things were tested out.

The patient's response to this combination of exposure with response prevention and "assertion training" was good and was maintained at twenty-four month follow-up. Her checking had remained markedly reduced and she reported herself to be more able to deal with the demands of others in a manner she felt to be appropriate.

A number of questions are raised by this case. Some patients are reported to relapse after discharge despite having made good progress in treatment with exposure and response prevention. Are some of these patients people who have failed to address such precipitating factors as situations involving others making demands upon them? As was evident from the case presented above, difficulties with such situations can sometimes remain very much alive after the symptoms they have evidently been precipitating have been reduced by exposure with response prevention. Similarly, are some of the patients who do not relapse those who are able, either for themselves or with the guidance of a therapist, to change their behaviour in interpersonal situations in the manner described, in addition to complying with the demands of their programmes of exposure with response prevention? And maybe a similar approach

would also help to shed light on some of the patients who fail to respond to exposure with response prevention at all - perhaps, that is, the interpersonal precipitants for some such cases are simply too strong for exposure with response prevention by itself to be of use? And maybe, therefore, the addition of assertion training to exposure with response prevention would improve the therapeutic outcome for some of these patients?

A final point to be made in connection with the case of the checking patient presented above is that it suggests yet another possible way in which exposure with response prevention may work, in at least some cases. As described above, exposure with response prevention appears in this case to have helped prevent the distress which had arisen elsewhere in the patient's life from being "converted into" her symptoms. This may, then, suggest the manner in which a psychodynamic account such as Malan's (1979) would attempt to explain why exposure with response prevention might be a successful intervention with some OCD patients, a task which Malan himself suggests to be important for such accounts. Janet's account of this success would perhaps most plausibly be along similar lines (also see section 3.4.7.3).

7.6 Further points and questions raised by Emmelkamp and van der Heyden's study

There are a number of further issues worth noting. Emmelkamp and van der Heyden's study has the virtue of attempting to manipulate - with assertion training - a variable which they hypothesise to be contributing to the production of "harming obsessions" - the unassertiveness of the patients suffering those obsessions - in order to see if these obsessions are reduced by this manipulation. Emmelkamp and van der Heyden's results mean that their hypothesis, and those aspects of Janet's and Malan's accounts which overlap with that hypothesis, are backed up by more than merely correlational or observational evidence. There is a regrettable absence of data of this kind for some of the approaches to OCD considered earlier, as was particularly noted in connection with Reed's account (see Chapter 4).

Emmelkamp and van der Heyden's findings also raise the question of whether the precipitants of at least some cases of OCD are primarily interpersonal in nature. They do not demonstrate that this is so - while interpersonal factors may no doubt be suggested to be an important precipitant of some cases of OCD by Emmelkamp and van der Hayden's study, such factors have not been shown to be of overriding importance. For example, an alternative position might suggest that situations in which these patients are required to assert themselves are simply highly anxiety provoking for them - this view would argue that it is this which precipitates the symptoms of these patients, and one would find them similarly affected by sources of anxiety not involving assertion or social interaction at all (for example anticipating surgery or dental treatment). (Emmelkamp and van der Heyden suggest that the contents of

harming obsessions indicate that anger is playing an important role in producing such obsessions, but symptom content can in itself surely not provide a conclusive argument for this [also see section 3.4.5.5], and in any case, it seems that not all of the obsessions which are provoked by the inappropriately unassertive behaviour of patients are harming obsessions, as has been argued above.)

This is an area which is suitable for further research, with two related issues standing in need of investigation. The first of these is the extent to which interpersonal difficulties are the most important precipitants of distress in OCD sufferers, and the second is the nature of the most important precipitating mood disturbance for those patients whose symptoms are provoked by interpersonal situations - and, in particular, whether this mood disturbance involves anxiety, anger or some combination of these, and whether there are still further types of mood disturbance also involved, for example depression and guilt? A growing literature on the nature of the mood disturbance among OCD patients in general has yet to provide a definitive answer to this question (for example, Beech and Liddell 1974, Rachman and Hodgson 1980, Reed 1985) and in the meantime, one is entitled to conclude that the hypothesis of unassertive behaviour in certain situations precipitating some obsessions is more strongly confirmed by the observations of Emmelkamp and van der Heyden than is the rather more specific suggestion that it is unexpressed aggressive feelings which precipitates these obsessions. Further phenomenological work is needed in order to support hypotheses which specify unexpressed aggressive feelings or anger to be the primary emotional disturbance in such cases, and Emmelkamp and van der Heyden's further claim that guilt about aggressive feelings helped to generate the obsessions of the patients they treated (and was reduced by assertion training) may be still more difficult to sustain. These authors present no evidence as to the levels of guilt experienced by their patients and similarly suggest no reason for believing that guilt concerning aggressive feelings should be reduced by the expression of aggressive feelings - on the face of things, it seems equally plausible to suggest that the expression of aggressive feelings would either increase or not change guilt concerning them.

7.7 Summary

Two main questions were raised in the introduction to this chapter - is there any evidence that assertion training can help those OCD patients who are inappropriately unassertive and, if so, is it possible to show that this effect is best explained by those accounts of OCD such as Emmelkamp and van der Heyden's (and those aspects of Janet's and Malan's accounts which coincide with it) which suggest that the unassertive behaviour of these OCD patients may be important in producing their symptoms? It was pointed out that the second

of these questions concerns whether or not in the present respect such accounts of OCD are "theoretically progressive", in the terms of Lakotos's and Putnam's work, while the first concerns whether or not they may also be said to be, in the terms of this work, "empirically progressive". One may tentatively offer an affirmative response to both questions, although further empirical and theoretical work in this area is required. Some suggestions as to further empirical work it might be worth conducting have also been offered.

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Appendix A: Control and background measures

Unequal variances were observed across the groups in the case of several of the control and background measures - the Beck Depression Inventory (BDI) (Cochrans C=0.45, p<0.01), the Sandler and Hazari (S+H) obsessional personality scale (Cochrans C=0.41, p=0.03), and symptom scale (Cochrans C=0.38, p=0.05), and the Maudsley Obsessive-Compulsive Inventory (MOCI) total score (Cochrans C= 0.50, p<0.01). Group 6 was therefore excluded from the analyses of the results on these measures, and the contrast results quoted are based upon separate variance estimates (see section 5.5.3).

Analyses of variance revealed significant overall group effects on the following measures: BDI (F[4,45]=11.26, p<0.01), the Eysenck Personality Questionnaire (EPQ) extraversion (E) (F[5,47]=2.45, p=0.05) and neuroticism (N) (F[5,47]=8.93, p<0.01) scales, all the MOCI subscales - checking (F[5,47]=12.61, p<0.01), cleaning (F[5,47]=16.86, p<0.01), slowness (F[5,47]=6.15, p<0.01), and doubting (F(5,47)=15.71, p=0.01) - and the MOCI total score (F[4,45]=39.53, p<0.01), the S+H personality scale (F[4,45]=12.20, p<0.01) and symptom scale (F[4,45]=15.12, p<0.01).

Significant overall group effects were not found on the Cognitive Failures Questionnaire (CFQ) (F[5,45]=1.41, p=0.24), the EPQ psychoticism (P) (F[5,47]=0.93, p=0.47) and lie (L) (F[5,47]=0.35, p=0.88) scales and the Mill Hill vocabulary scale (MH) (F[4,45]=0.33, p=0.86).

What is being termed here the "Doubt, Repetition and Relief" (DRR) questionnaire was introduced by Rachman and Hodgson (1980) to measure differences in the degree of doubting, repetitiveness and relief from discomfort associated with cleaning compulsions, on the one hand, and checking compulsions, on the other. Rachman and Hodgson found cleaning compulsions to be associated with less doubting and repetitiveness, and more relief from discomfort, than checking compulsions. The cleaners (group 1) and checkers (group 2) in the present investigation were, however, indistinguishable on the questionnaire (F[1,18]=0.90, p=0.36).

The group means and standard deviations for all of the control and background measures are presented in tables A.1-A.4. The same contrasts as were specified for the results reported in chapters 5 and 6 were also examined for the control and background measures, and the results of these are presented in tables A.5.1-A.6.2. The psychiatric and normal controls did not differ significantly from one another on the measures included in tables A.5.1 and A.5.2, and are therefore combined in the results reported on that table to form a single group of non OCD controls. The psychiatric controls did differ significantly from the normal controls

on all of the results included in tables A.6.1 and A.6.2, meaning that no such single group of non OCD controls could not be formed here. The subjects in group 6 differed significantly from the other OCD groups on one measure, the MOCI cleaning subscale, and these subjects were therefore excluded from the combined group of all OCD patients on this measure.

On those measures where there were significant differences between the cleaners (group 1) and checkers (group 2), they were not combined to form a single group of OCD patients, and so no contrast results are reported for "All OCD groups" on these measures.

The following abbreviations are used in the tables: "psych ctls" = psychiatric controls, "normal ctls" = normal controls, "non ch/cl" = non checking/cleaning patients (i.e. group 6 - see section 5.5.2.1), "non OCD controls" = non OCD controls (i.e. groups 4 and 5 combined - see section 5.5.2.1).

Table A.1: The means and standard deviations of the scores of all the groups on the Eysenck Personality Questionnaire (EPQ), extraversion (E), neuroticism (N), psychoticism (P), and lie (L) scales

	EPQ (E)	EPQ (E)	EPQ (N)	EPQ (N)	EPQ (P)	EPQ (P)	EPQ (L)	EPQ (L)
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Cleaners (N=10)	6.80	4.18	19.30	2.45	3.20	2.29	7.30	5.40
Checkers (N=10)	12.70	5.77	16.90	4.25	2.90	1.79	7.90	4.25
Ex OCD pts (N=10)	8.30	3.68	14.80	6.03	3.40	2.76	8.20	5.49
Psych ctls (N=10)	9.20	5.05	16.50	5.21	3.50	2.37	8.70	5.31
Norm ctls (N=10)	12.30	6.45	6.40	5.17	4.40	1.84	6.50	4.67
Non ch/cl (N=3)	7.00 *	5.66	18.50 *	6.36	5.50 *	0.70	9.0 *	4.24

* One case missing

Table A.2: The means and standard deviations of the scores of all the groups on the Cognitive Failures Questionnaire (CFQ), the Beck Depression Inventory (BDI), the Doubting, Repetition and Relief (DRR) questionnaire and the Mill Hill (MH) vocabulary scale.

	CFQ	CFQ	BDI	BDI	DRR	DRR	MH	MH
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Cleaners (N=10)	51.80	18.24	24.90	11.19	24.00	6.16	19.0	2.21
Checkers (n=10)	41.90	12.59	13.60	7.09	26.10	3.35	18.90	3.51
Ex OCD pts (N=10)	41.00	18.86	7.90	7.64			20.7	6.18
Psych ctls (n=10)	37.33*	18.06	14.40	8.07			20.20	5.73
Norm ctls (n=10)	33.44*	12.58	2.78 *	3.46			20.33 *	3.71
Non ch/cl (N=3)	41.00	1.73	18.67	16.70			20.00	7.94

* One case missing

Table A.3: The means and standard deviations of the scores of all the groups on the Maudsley Obsessive-Compulsive Inventory (MOCI), total score (t), and checking (ch), cleaning (cl), slowness (sl), doubting (dt) subscales

	MOCI(ch)	MOCI(ch)	MOCI(cl)	MOCI(cl)	MOCI(sl)	MOCI(sl)	MOCI(dt)	MOCI(dt)	MOCI(t)	MOCI(t)
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Cleaners (N=10)	5.70	2.54	9.10	2.28	3.20	1.40	5.70	1.64	23.40	5.76
Checkers (N=10)	6.30	1.34	3.50	2.42	4.40	1.67	5.60	0.97	18.80	3.58
ex OCD (N=10)	3.80	3.22	3.00	3.20	2.30	1.57	3.50	2.07	11.50	5.19
Psych ctls (N=10)	1.70	2.06	1.80	2.15	1.50	0.71	3.20	1.75	7.20	5.25
Norm ctls (N=10)	0.0	0.0	0.2	0.42	1.90	0.74	0.70	0.67	1.0	0.82
Non ch/cl (N=3)	6.00	3.00	2.33	3.21	2.67	2.08	5.67	1.53	16.33	10.07

Table A.4: The means and standard deviations of the scores of all the groups on the Sandler and Hazari (S+H) obsessional personality (p) and symptoms (s) scale

	S+H (p)	S+H (p)	S+H (s)	S+H (s)
	Mean	**SD**	**Mean**	**SD**
Cleaners (N=10)	12.20	2.53	13.30	4.74
Checkers (N=10)	11.80	2.35	11.60	2.54
Ex OCD pts (N=10)	8.60	4.06	7.90	3.87
Psych ctls (N=10)	7.50	3.72	5.90	4.15
Norm ctls (N=10)	4.00	2.05	2.00	2.40
Non ch/cl (N=3)	9.00 *	5.66	12.50 *	6.36

* One case missing

Table A.5.1: Contrast results for Maudsley Obsessive-Compulsive Inventory (MOCI) cleaning (cl), checking (ch) and slowness (sl) subscales, the Eysenck Personality Questionnaire (EPQ) extraversion (E), psychoticism (P) and lie (L) scales, the Cognitive Failures Questionnaire (CFQ) and the Mill Hill (MH) vocabulary scale - OCD groups and controls

	MOCI(cl)	MOCI(ch)	MOCI(sl)	EPQ(E)	EPQ(P)	EPQ(L)	CFQ	MH
All OCD groups vs non OCD ctls		t=7.14, p<0.01			t=-0.19, p=0.85	t=0.49, p=0.63	t=1.76 with 45 df, p=0.09	t=-1.03, p=0.31
Checkers vs cleaners	t=5.37, p<0.01	t=-0.62, p=0.50	t=-2.03, p=0.05	t=-2.58, p=0.01	t=0.31, p=0.76	t=-0.27, p=0.79	t=1.39 with 45 df, p=0.17	t=0.08, p=0.94
Checkers vs non OCD ctls	t=2.77, p<0.01	t=6.45, p<0.01	t=5.27, p<0.01	t=0.98, p=0.33	t=-1.24, p=0.22	t=0.16, p=0.88	t=0.13 with 45 df, p=0.90	t=-0.88, p=0.39
Cleaners vs non OCD ctls	t=8.97, p=0.01	t=5.74, p=0.01	t=2.93, p<0.01	t=-12.99, p=0.05	t=-0.88, p=0.38	t=-0.16, p=0.88	t=2.60 with 45 df, p=0.01	t=-0.97, p=0.34

All t values quoted have 47 df unless otherwise stated

Table A.5.2: Contrast results for Maudsley Obsessive-Compulsive Inventory (MOCI) cleaning (cl), checking (ch) and slowness (sl) subscales, the Eysenck Personality Questionnaire (EPQ) extraversion (E), psychoticism (P) and lie (L) scales, the Cognitive Failures Questionnaire (CFQ) and the Mill Hill (MH) vocabulary scale - ex OCD patients, OCD groups and controls

	MOCI(cl)	MOCI(ch)	MOCI(sl)	EPQ(E)	EPQ(P)	EPQ(L)	CFQ	MH
Ex OCD pts vs all OCD pts		t=2.53, p=0.02			t=0.47, p=0.64	t=0.10, p=0.92	t=0.61 with 45 df, p=0.54	t=-0.85, p=0.41
Ex OCD pts vs non OCD ctls	t=2.22, p=0.03	t=3.49, p<0.01	t=1.17, p=0.25	t=-1.24, p=0.22	t=-0.65, p=0.52	t=0.31, p=0.76	t=0.89 with 45 df, p=0,38	t=-0.19, p=0.85
Ex OCD pts vs checkers	t=0.48, p=0.63	t=2.56, p=0.02	t=3.55, p<0.01	t=1.92, p=0.06	t=-0.51, p=0.61	t=-0.13, p=0.89	t=0.13 with 45 df, p=0.90	t=-0.80, p=0.44
Ex OCD pts vs cleaners	t=5.85, p<0.01	t=1.95, p=0.06	t=1.52, p=0.14	t=-0.66, p=0.52	t=-0.20, p=0.84	t=-0.40, p=0.69	t=1.51 with 45 df, p=0.14	t=-0.82, p=0.43

All t values quoted have 47 df unless otherwise stated

Table A.6.1: Contrast results for the Maudsley Obsessive-Compulsive Inventory (MOCI) doubting (dt) subscale and total (t) score, the Eysenck Personality Questionnaire (EPQ) neuroticism (N) scale, the Sandler and Hazari obsessional personality (p) and symptoms (s) scale, and the Beck Depression Inventory (BDI) - OCD groups and controls

	MOCI(dt)	MOCI (t)	EPQ (N)	S+H (p)	S+H (s)	BDI
All OCD vs psych ctl	t=7.03, with 16.4 df p<0.01		t=0.88, p=0.38	t=3.47 with 13.0 df, p=0.04	t=4.19 with 16.3 df, p<0.01	
All OCD vs norm ctl	t=18.22, with 16.8 df, p<0.01	t=8.90 with 2.7 df, p=0.01	t=6.19, p<0.01	t=9.23 with 20.9 df, p<0.01	t=9.16 with 22.6 df, p<0.01	t=7.13 with 21.7 df, p<0.01
Checkers vs cleaners	t=0.15, p=0.88	t=2.15 with 15.1 df, p=0.05	t=1.13, p=0.27	t=0.37 with 17.9 df, p=0.72	t=0.99 with 13.8 df, p=0.34	t=2.70 with 15.2 df, p=0.02
Checkers vs psych ctl	t=3.55, p<0.01	t=5.77 with 15.9 df, p<0.01	t=0.19, p=0.85	t=3.09 with 15.2 df, p<0.01	t=3.70 with 14.9 df, p=0.01	t=-0.24 with 17.7 df, p=0.82
Checkers vs norm ctls	t=7.25, p<0.01	t=15.31 with 9.9 df, p<0.01	t=4.92, p<0.01	t=7.91 with 17.7 df, p<0.01	t=8.67 with 17.9 df, p<0.01	t=4.47 with 12.9 df,p<0.01
Cleaners vs psych ctls	t=3.70, p<0.01	t=6.58 with 17.8 df, p<0.01	t=1.31, p=0.20	t=3.30 with 5.9 df, p<0.01	t=3.72 with 17.7 df, p<0.01	t=2.41 with 16.4 df, p=0.03
Cleaners vs norm ctls	t=7.40, p<0.01	t=12.18 with 9.4 df, p<0.01	t=6.05, p=0.01	t=7.96 with 17.3 df, p<0.01	t=6.73 with 13.3 df, p<0.01	t=6.06 with 10.6 df, p<0.01

All t values quoted have 47 df unless otherwise stated

Table A.6.2: Contrast results for the Maudsley Obsessive-Compulsive Inventory (MOCI) doubting (dt) subscale and total (t) score, the Eysenck Personality Questionnaire (EPQ) neuroticism (N) scale, the Sandler and Hazari obsessional personality (p) and symptoms (s) scale, and the Beck Depression Inventory (BDI) - ex OCD patients, OCD groups and controls

	MOCI (dt)	MOCI (t)	EPQ (N)	S+H (p)	S+H (s)	BDI
All OCD pts vs ex OCD pts	t=4.90 with 16.5 df, p<0.01		t=1.77, p=0.08	t=2.44 with 12.3 df, p=0.03	t=3.05 with 17.2 df, p<0.01	
Ex OCD pts vs psych ctls	t=0.44, p=0.66	t=1.84 with 18.0 df, p=0.08	t=-0.80, p=0.43	t=0.63 with 17.9 df, p=0.54	t=1.12 with 17.9 df, p=0.28	t=-1.85 with 17.9 df, p=0.08
Ex OCD pts vs norm ctls	t=-4.14, p<0.01	t=6.32 with 9.4 df, p<0.01	t=-3.94, p<0.01	t=3.20 with 13.3 df, p<0.01	t=4.09 with 15.0 df, p<0.01	t=2.05 with 12.4 df, p=0.06
Ex OCD pts vs checkers	t=3.11, p=0.01	t=3.66 with 16.0 df, p<0.01	t=0.98, p=0.33	t=2.16 with 14.4 df, p=0.05	t=2.53 with 15.6 df, p=0.02	t=1.73 with 17.9 df, p=0.10
Ex OCD pts vs cleaners	t=3.26, p<0.01	t=4.85 with 17.8 df, p<0.01	t=2.11, p=0.04	t=2.38 with 15.1 df, p=0.03	t=2.80 with 17.3 df, p=0.01	t=3.97 with 15.9 df, p<0.01

All t values quoted have 47 df unless otherwise stated

Appendix B: Consent Form

Informed Consent

This is to certify that I have been informed of the nature and aims of this clinical study. I freely consent to take part, and understand that I am free to withdraw at any time

Patient's signature Date

Investigator's signature Date